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AND

DISCUSSIONS

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AMERICAN SOCIETY OF CIVIL ENGINEERS
Founded November 5, 1852
PAPERS

TECHNICAL SURVEY—BROOKLYN BRIDGE AFTER SIXTY YEARS

A SYMPOSIUM

Note.—Written comments are invited for immediate publication; to insure publication the last discussion should be submitted by **June 1, 1946.**

Foreword

The Brooklyn Bridge is a suspension structure that crosses the East River between the Borough of Manhattan and the Borough of Brooklyn, in New York, N. Y. In 1943, when an examination of its physical condition and load-bearing capacity became desirable, it was 60 years old.

This investigation, which is described in the Symposium, was conducted by the Department of Public Works, City of New York, of which Irving V. A. Huie, M. ASCE, was then commissioner. Homer R. Seely, M. ASCE (author of the first paper), had general charge of the investigation for the Department, which was made under the guidance of a Board of Consultants comprising in addition to O. H. Ammann, M. ASCE (author of the second paper), the late Holton D. Robinson and the late Leon S. Moisseiff, Members, ASCE. The so-called technical survey was implemented by members of the engineering staff of the Department under the direction of Isidore Delson, M. ASCE, with Nomer Gray, Assoc. M. ASCE (author of the third paper), in charge of the work in the field. The tests described in the fourth and last paper were made under the direction of Harold E. Wessman, M. ASCE (author of the fourth paper).

HISTORICAL FEATURES OF CONSTRUCTION AND OPERATION

By Homer R. Seely,[1] M. ASCE

Preliminary Studies

While describing the anchorages of the Brooklyn Bridge across the East River between Brooklyn and New York, N. Y., in his progress report of 1876, Col. Washington A. Roebling made the following statement:

> "In European suspension bridges, the chains are usually carried in tunnels open to access, which are confidingly left to the care of future generations, and this trust is not betrayed. This, however, is contrary to the genius of the American people, with whom everything has to look out for and take care of itself; hence, in the arrangement of this anchorage, the chains are inaccessibly preserved and are not intrusted to the neglect of posterity."

In May, 1945, the Brooklyn Bridge had withstood the ravages of time and use for 62 years. How well the bridge has survived the "neglect of posterity" and how well it can serve the traffic of the future is the subject of this Symposium. This paper is devoted to a brief history of the bridge and a review of some of the more interesting features of its construction and operation.

The Brooklyn Bridge (or New York and Brooklyn Bridge as it was officially called in those days), like all undertakings of great magnitude, had been agitated, on occasion, since the early years of the nineteenth century. However, it remained for the action of three men—Henry C. Murphy, then a New York State Senator, William C. Kingsley, F. ASCE, and Alexander McCue, who met on December 21, 1866, in the City of Brooklyn—to reach an agreement resulting in the passage of an act by the New York State Legislature on April 16, 1867, incorporating the New York Bridge Company for the purpose of building and maintaining such a bridge. This action was undoubtedly aided by the unusually severe weather that winter, which caused the East River to freeze. As a result, it was possible to travel from Albany to New York by railroad in less time than it took the business man living in Brooklyn to reach his office in downtown Manhattan.

The corporators of the New York Bridge Company first met on May 13, 1867, and on May 23 retained John A. Roebling, M. ASCE, as their chief engineer. Mr. Roebling was admirably fitted for this assignment—ten years previously he had accomplished the proclaimed "impossible" by constructing a railway suspension bridge over the Niagara gorge at Niagara Falls, N. Y., and earlier that same year (1867) had completed the Cincinnati (Ohio)-Covington (Ky.) Bridge over the Ohio River with a record span of 1,057 ft.

Mr. Roebling immediately proceeded with the necessary surveys, preliminary plans, and estimates of cost and submitted his initial report on September 1, 1867. He investigated three locations, recommending the south

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[1] Deputy Commr., Dept. of Public Works, New York, N. Y.

or City Hall Park location which was ultimately adopted (see Fig. 1). The river crossing required a clear span of approximately 1,600 ft. In his report, Mr. Roebling[2] stated:

> "Any span inside of 3,000 feet is practicable. With the best quality of steel wire, even greater spans may be made secure for all kinds of traffic; but, of course, the cost of such works increases with the length of span."

His implicit faith in the safety of a bridge of this magnitude, however, was not shared by a large number of the citizens. Therefore, he requested and re-

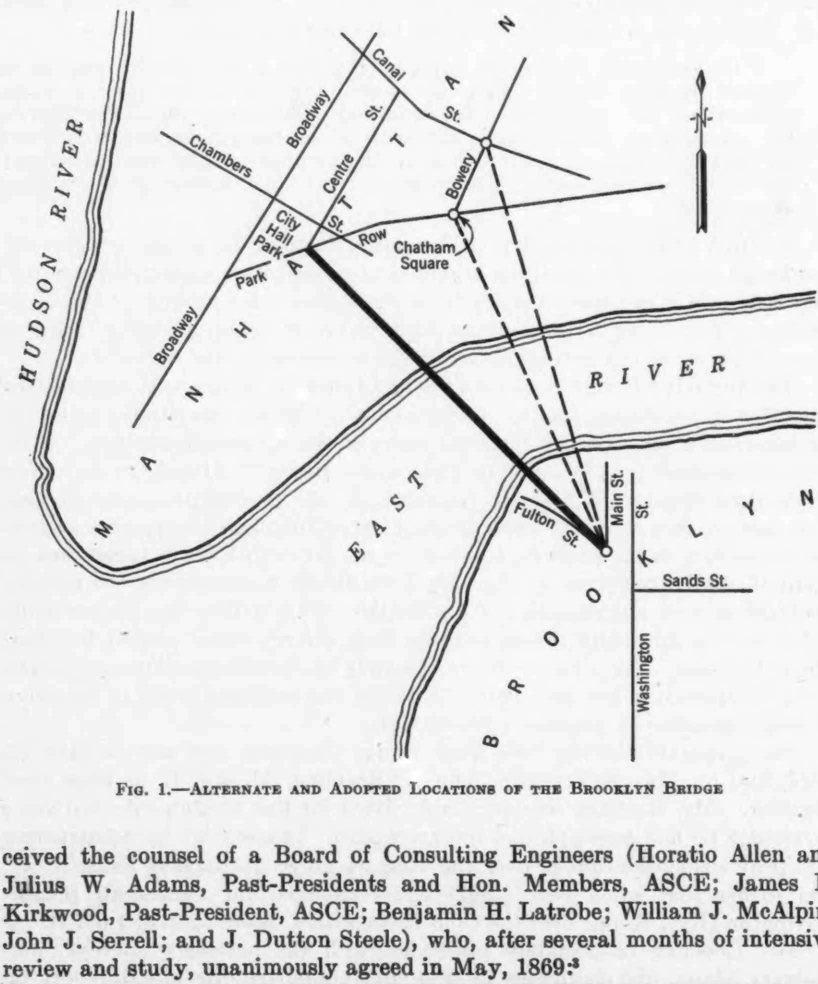


FIG. 1.—ALTERNATE AND ADOPTED LOCATIONS OF THE BROOKLYN BRIDGE

ceived the counsel of a Board of Consulting Engineers (Horatio Allen and Julius W. Adams, Past-Presidents and Hon. Members, ASCE; James P. Kirkwood, Past-President, ASCE; Benjamin H. Latrobe; William J. McAlpin; John J. Serrell; and J. Dutton Steele), who, after several months of intensive review and study, unanimously agreed in May, 1869:[3]

[2] "Report of John A. Roebling, C. E., to the President and Directors of the New York Bridge Company on the Proposed East River Bridge," Eagle Book and Job Printing Dept., Brooklyn, N. Y., 1867, p. 8.

[3] "Brooklyn Bridge 1883–1933," Dept. of Plant and Structures, New York, N. Y., 1933, p. 8.

"It is beyond doubt entirely practical to erect a steel wire suspension bridge of 1,600 feet span, 135 feet elevation, across the East River, in accordance with the plans of Mr. Roebling, and that such a structure will have all the strength and durability that should attend the permanent connection by a bridge of the Cities of New York and Brooklyn."

An Act of Congress (Public No. 53), approved on March 3, 1869, legalized the bridge as a post road for the conveyance of mails of the United States provided that it was constructed so as to not "obstruct, impair or injuriously modify the navigation of the river," and, to secure compliance with these conditions, the act stipulated that the plans of the bridge be submitted for the approval of the Secretary of War. A commission of three U. S. Army Engineers (Lt.-Col. H. G. Wright and Lt.-Col. John Newton, Hon. Members, ASCE, and Capt. W. R. King) was designated to report on the structure. After an exhaustive investigation, it was recommended that the bridge (with an underclearance of 130 ft) be approved, as planned. The recommendation was also endorsed by Brig.-Gen. A. A. Humphreys, Hon. M. ASCE, then Chief of Engineers, U. S. Army. However, in granting his approval on June 21, 1869, John A. Rawlins, then Secretary of War, acceded to the navigation interests and stipulated that the clear height of the bridge at the center of the main span be not less than 135 ft above "mean high water of the spring tides." It was recognized that even at this height it would be necessary for some of the larger merchant vessels, in order to sail under the bridge, to "send down or house their royals, and in some cases, their top gallant masts." Even so, for many years after the bridge was completed, the annual reports state that several ships were deprived of their topmasts each year.

Final surveys for constructing the bridge were started in June, 1869, but fate decreed that its master was not to build the bridge, as, on June 28, John A. Roebling, while standing on a fender rack, had his right foot crushed by a ferryboat approaching its slip. Tetanus developed and he died on July 22. Fortunately, associated with him on the work was his son, Col. Washington A. Roebling. In 1865, when only twenty-eight Colonel Roebling had resigned his commission in the Union Army to assist his father in completing the Cincinnati Bridge, taking practically complete charge of the construction. Obviously, in spite of his age, he was the logical successor to the position of chief engineer, and he was so appointed on August 3, 1869.

The trials and tribulations which confronted Colonel Roebling and his associates in the construction of the bridge were enough to exhaust the patience of ordinary men. The problems which arose in the sinking of the pneumatic caisson for the Brooklyn tower alone would furnish sufficient interesting material for a paper.

Foundation Problems

Actual construction started in January, 1870, at the site of the Brooklyn tower. The site selected for the tower fell upon a glacial deposit which proved to be a mass of boulders cemented together with decomposed fragments of green serpentine rock, making them almost impossible to dislodge. These boulders ranged in size from 1 cu ft to larger than 300 cu ft. A collection of the various

kinds of boulders encountered during the sinking of the caisson included all the varieties of rock found for a hundred miles north and northeast of Brooklyn; 90% of the boulders were of traprock, most likely originating in the Palisades.

Of course, it was necessary to break up the larger boulders in order to remove them from the working chamber. At first, this was done by plug and feather, but some more expedient method soon became necessary. Blasting had been considered from the beginning, but it was feared that, in addition to rupturing the eardrums of the workmen, a violent explosion in the compressed air of the working chamber would cause disastrous results to the caisson. After experimenting at first with a pistol, using successively heavier charges and then still larger charges fired by fuse, it was found that blasting caused no harmful effects. The powder smoke was quite a nuisance, however, as it would fill the working chamber for a half hour or more and obscure the lights. The sulfur odor did not matter as the sense of smell practically disappears in compressed air.

Illumination of the working chambers was a difficult problem. The general illumination was furnished by calcium lights. Care had to be taken, however, to prevent explosions caused by leaking gas or carelessness in turning off the burners. One explosion did take place, which, according to Colonel Roebling was "sufficient to singe off whiskers and create some alarm." Candles were used for special work where extra light was necessary. Incomplete combustion of the candles resulted in an intolerable amount of smoke in spite of all precautions, such as reducing the size of wick as well as soaking the wick in vinegar and mixing alum with the tallow. The smoke was very injurious to the lungs of the workmen; six months after the caisson work was completed, the workmen still coughed up sputa streaked with carbon black.

In the Brooklyn caisson the men worked in 8-hr tours, including 1 hour for meals, which they usually ate in the working chamber. Their wages were $2.00 a day, which was increased to $2.25 when the caisson had reached a depth of 28 ft. On the New York side, because of the greater depth and correspondingly greater pressure, the daily working hours were gradually reduced from two 4-hr tours to two 2-hr tours. The wage scale was also increased to $2.75 a day.

As one might expect, caisson disease, or the "bends," presented an acute problem, particularly on the New York side. According to Colonel Roebling, "scarcely any man escaped without being somewhat affected by intense pain in his limbs or bones or by a temporary paralysis of arms or legs." Full advantage was taken of all prior experience and knowledge of the subject—Colonel Roebling having previously made a special trip to Europe to learn of new developments in the use of pneumatic caissons. In addition, Andrew H. Smith, M.D., was retained to attend those afflicted and to conduct research. In his report Dr. Smith stated that, during his employment on the work, there were 110 cases that required treatment, of which three proved fatal. Colonel Roebling, himself, was a victim, being permanently crippled and confined to his home, from which point he continued to direct the work through his loyal and capable assistants. Some question was raised as to his ability to continue in charge of the work. A resolution introduced by Seth Low, then Mayor of

New York City, to the Board of Trustees proposed that he be appointed as consulting engineer because:

> "In the judgment of this Board the absence of the Chief Engineer from the post of active supervision is necessarily, in many ways, a source of delay * * *."

Colonel Roebling positively refused to consider such an appointment and requested: "* * * that the vote of the board be taken simply as to whether or not I am to remain chief engineer." After a prolonged and heated debate, the resolution failed to be adopted by a vote of 10 to 7. In his defense Colonel Roebling also prepared a paper which Mrs. Roebling read before a meeting of the Society. There is no record, however, that the Society took any official action in his behalf. The ire of some of the Trustees was undoubtedly aroused by Colonel Roebling's inability to comply with the Board's request to attend one of its meetings when he telegraphed curtly: "Cannot meet the Trustees today." To their repeated requests, he wrote:

> "I am not well enough to attend the meetings of the Board as I can talk for only a few moments at a time and cannot listen to conversation if it is continued very long. * * * I did not telegraph you before the last meeting that I was sick and could not come, because everyone knows I am sick and they must be as tired as I am of hearing my health discussed in the newspapers."

Colonel Roebling resigned from the position of chief engineer on June 30, 1883, shortly after the bridge was opened to traffic.

The caissons were constructed of yellow pine with 2-ft square oak sills resting on a semicircular casting, forming the cutting edge. The caissons were 102 ft wide and approximately 170 ft long—by far the largest pneumatic caissons ever constructed to that time.

The manways and supply shafts to the working chambers were equipped with air locks (see Fig. 2). The relative position of the air locks on the manway shafts became the subject of a suit for infringement of patent by Capt. James B. Eads, M. and F. ASCE, builder of the well-known Eads Bridge at St. Louis, Mo. In writing about this subject in the technical press,[4] Captain Eads stated:

> "I trust I shall not be understood as finding fault with Colonel Roebling for copying my plans in his New York caisson. * * * Colonel Roebling's failure for the past three years to credit me with the plans appropriated by him was not deemed of sufficient moment to cause me to trouble the public with the matter but having frequent occasion myself for them when designing foundations of other works, his omission being now coupled with an effort to deprive me of all merit of originality in them, compels me most reluctantly to correct his statements and show my right to use my own property."

In rebuttal, Colonel Roebling wrote:[5]

> "Captain Eads virtually makes the broad claim that any device which has been used in a pneumatic cylinder can be made the subject of a new

[4] *Engineering* (London), Vol. 15, May 16, 1873, p. 337.

[5] *Ibid.*, June 27, 1873, p. 458.

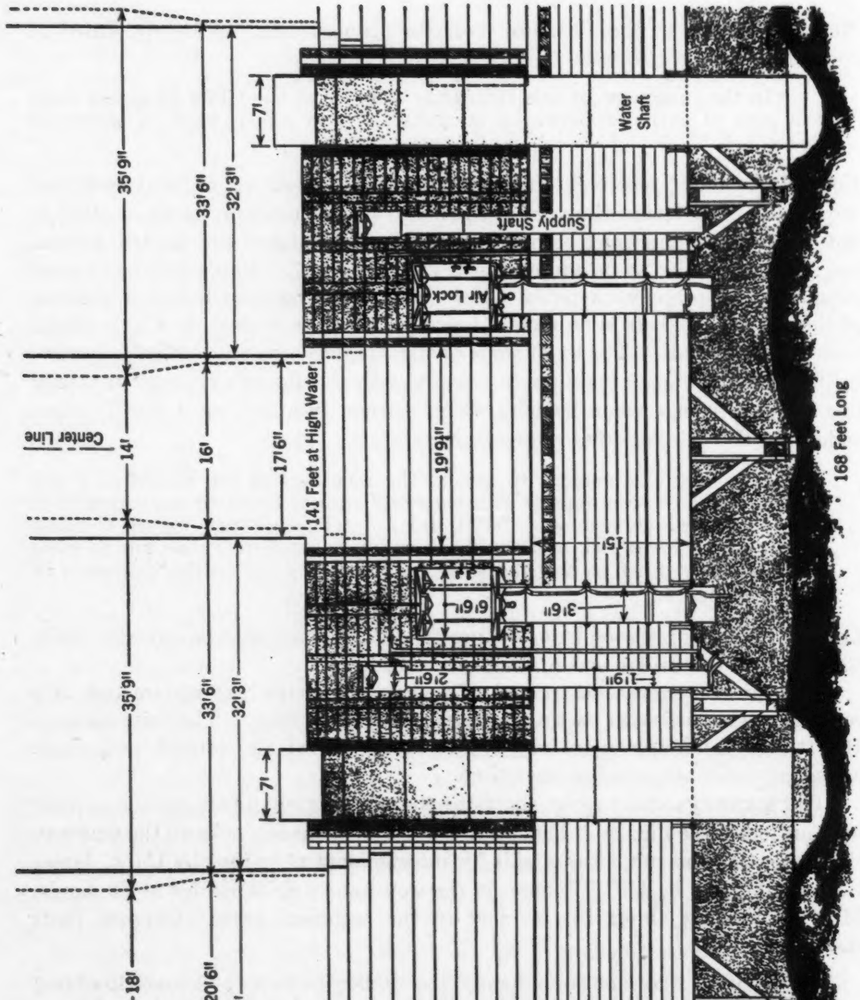


FIG. 2.—LONGITUDINAL SECTION, BROOKLYN TOWER CAISSON

patent when applied to a 'masonry caisson' and in that spirit has had several patents granted. I choose to differ with him on that point and before paying the round sum with which he proposes to tax the engineering world for the next fifteen years, I have preferred to leave the matter to the decision of the court at law where it is now being tried."

A resolution adopted in 1876 by the Board of Trustees of the bridge indicates that the suit was settled in favor of Captain Eads.

"Water shafts" (see Fig. 3) were used for removing the excavated material. There were two shafts, entirely open from top to bottom, with the lower ends submerged in pools of water. The air pressure in the working chamber was balanced by columns of water extending up the shafts. The men in the working chambers excavated the material and deposited it in the pools of water where it was picked up and hoisted up the shafts by a clamshell bucket, which was apparently a patented device in those days and called a "Grapnel" bucket.

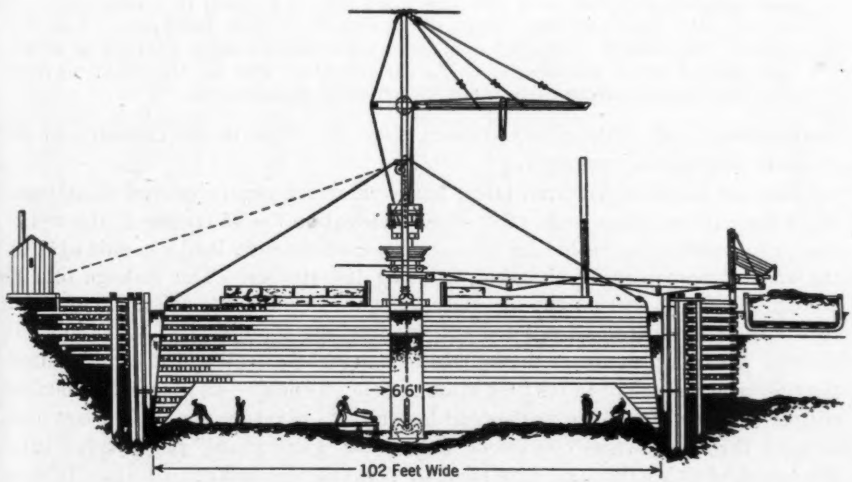


Fig. 3.—Arrangement of Excavating Machinery, Brooklyn Tower Caisson

Occasionally, a stone would prevent the bucket from closing properly, and the men would dive into the pool and clear the obstruction. With compressed air in their lungs, they were able to stay under water 3 min or 4 min with little difficulty. Contrary to expectations, it was found that the buckets alone were not able to excavate enough material under the shafts to maintain the pools so that, periodically, it was necessary to discontinue operations, pump out the pools, and excavate the material by hand. To maintain the air pressure, a cover was bolted on the shaft, loaded with rock to balance the air pressure, and the water in the shaft was released by admitting compressed air into the top of the shaft. One or two days were lost each time this operation became necessary.

During the sinking of the Brooklyn caisson, an incident occurred which Colonel Roebling refused to admit was an accident; but, rather,

"* * * simply the legitimate result of carelessness brought about by an over-confidence in supposing that matters would take care of themselves."

Early one Sunday morning, one of the earth dams retaining the water in the pools under the water shafts was washed away, allowing the air to escape through the shaft. The incident was recorded in Col. Washington Roebling's report as follows:[6]

"Eye-witnesses outside state that a dense column of water, fog, mud and stones was thrown up five hundred feet into the air, accompanied by a terrific roar and a shower of falling fragments, covering the houses for squares around. This column was seen a mile off. The noise was so frightful that the whole neighborhood was stampeded and made a rush up Fulton Street. Even the toll-collectors at the ferry abandoned their tills. There were three men on the caisson at the time, including the watchman. He reports that the current of air rushing toward the blowing water shaft was so strong as to knock him down; while down, he was hit on the back by a stone, and further than that he does not remember. One of the other men jumped into the river and the third buried himself in a coal pile. It was all over in a minute. Both doors of the air-lock fell open. The dry bottom was visible through the air and water shaft; not a particle of water had entered under the shoe into the air chamber, and for the first and only time the caisson could dispense with artificial illumination."

Fortunately, very little, if any, real damage was done to the caisson and air pressure was quickly restored.

Another incident occurred later, however, which nearly proved disastrous. With the cutting edges within 2 ft of final elevation (− 45 ft) one of the workmen, while retrieving his hidden dinner pail, inadvertently held a candle against the roof of the working chamber, igniting the timber. The leakage of air, acting as a draft, carried the fire zigzagging up through the caisson roof. All efforts to extinguish the fire failed and the working chamber had to be flooded. Colonel Roebling remained in the caisson all night directing the fight against the fire, which resulted in his first attack of the "bends"—an illness destined to cripple him for life. The fire spread laterally to points some 50 ft apart and upward through several courses of timber. Cement grout was injected into the burnt cavities through boreholes to prevent the leakage of air. It was hoped that this might also serve to repair the damage; but, since further inspection indicated the presence of considerable quantities of charred wood, it was necessary to cut out the burned timbers progressively and to replace them or to scrape away the charred portions and pack the cavities with cement mortar. The repair work was particularly disagreeable:

"* * * the men having to lie for hours in confined spots without room to turn, and breathing a foul mixture of candle smoke and cement dust, combined with powdered charcoal * * *."

The New York tower caisson, although founded at nearly twice the depth of the Brooklyn tower (− 78 ft) was sunk in 20% less time. This was largely because the material excavated was principally sand, which was blown to the surface through pipes with the compressed air in the working chamber. Some

[6] "Report of the Chief Engineer and General Superintendent to the Board of Directors of the New York Bridge Company," Eagle Book and Job Printing Dept., Brooklyn, N. Y., 1871, p. 20.

difficulty was encountered in finding a material that would deflect the sand at the top—otherwise the sand would have shot some 400 ft into the air. Wrought-iron and cast-iron elbows, used at first, would last only 1 hour or 2 hours. The elbows were finally discarded and heavy blocks of granite were hung over the pipes. The pebbles discharged with the sand, however, proved quite a hazard—a boatman on the river had a finger shot off and one of the laborers working on the caisson was shot through the arm. Comparatively little material was excavated through the "water shafts"; in fact, they were capped and not used at all during the last 10 ft of excavation.

Cable Spinning

The masonry towers and anchorages were completed sufficiently to start cable construction in August, 1876. One of the traveling or "hauling" ropes and a "carrier" rope were erected by first laying them on the bed of the river

Fig. 4.—Main Cables Under Construction

and then hoisting them to the top of the towers. The remaining ropes were erected by hauling them across the river supported on hangers running on the carrier rope. Only one footbridge, 3.5 ft wide (see Fig. 4), was used to give access to the different parts of the work. At three points in the main span and the midpoint of each land span, "cradles" or crosswalks were erected to give access from the footbridge to the wire adjusting platforms. The cable strands were spun approximately 60 ft above their final position at the center of the main span to insure the wire being pulled straight under the correspondingly higher tension. Also, the greater underclearance of the temporary ropes

offered less interference to navigation for a considerable added length of time. Hanging platforms were erected for adjusting the strands which were reached by ladders from the wire adjusting platforms.

The cable wire was spun basically the same as for modern bridges. However, only one strand for each cable was spun at a time, so that the spinning wheels returned empty each trip. Eighteen months were required to spin the cables, the last wires being placed on October 5, 1878. Based on his experience at Cincinnati, Colonel Roebling had estimated that it would take at least 2.5 years to 3 years to spin the cables. Only one major mishap occurred on this phase of the work. While one of the strands was being slacked off to adjust it to the proper sag, the strand broke loose at the New York anchorage and slipped over the tower until it came to rest on the bed of the river. Fortunately, no boat happened to be under the bridge or it might have been cut in half; but, unfortunately, the strand shoe did strike and kill two of the men working on the anchorage.

Difficulties of Material Inspection

Galvanized steel wire was used for the main cables instead of iron wire which had been used for all previous parallel wire bridge cables. The specifications established limitations as to unit weight, strength, elastic limit, and modulus of elasticity. However, no restrictions were included on the process of manufacturing the steel or the wire even though at the time it was generally believed that open hearth or Bessemer steel would not produce acceptable wire. All parties expecting to submit a bid were requested to furnish samples of the wire they proposed to supply. Colonel Roebling realized that this was no assurance that all the wire would conform to the sample, but (he indicated) it would serve to acquaint each bidder with all the problems involved in producing the wire and convince him that there were no unjust or arbitrary conditions in the specifications, that would be impossible to fulfil. The samples submitted by prospective bidders soon indicated:[7]

> "* * * that both Open Hearth and Bessemer steels could be so manipulated as to meet the requirements in every respect, giving *uniformity* equal to and even superior to any brand of cast-steel * * *."

John A. Roebling's Sons Company submitted alternative bids for wire of crucible cast steel and of Bessemer steel. The Executive Committee recommended the award of the contract to the Roebling Company on the basis of their low bid for Bessemer steel wire. Nevertheless, a strong vote of the Board of Trustees in favor of crucible cast-steel wire caused the contract finally to be awarded to the firm submitting the low bid for such wire. The contract price was 8.7¢ a lb—nearly 2¢ a lb more than the Roebling bid. The bids were taken at so much per pound, gold, to eliminate all questions of probable changes in the value of currency during the life of the contract.

The successful bidder was a local Brooklyn company and everything apparently went well until inspectors noticed that the stock of rejected wire,

[7] "Report of the Chief Engineer of the New York and Brooklyn Bridge, January 1, 1877," Eagle Book and Job Printing Dept., Brooklyn, N. Y., 1877, p. 21.

instead of growing larger, was gradually growing smaller. It was determined that the contractor was substituting rejected wire for all approved wire which happened to remain at the plant overnight and then, of course, on the following morning, carted it to the bridge without further inspection. When this skulduggery was discovered, the contractor conceived the idea of having each wagonload of wire driven to a building en route where the approved wire was replaced with an equal weight of rejected wire. The approved wire was then returned to the plant and resubmitted to the inspector as new wire. Colonel Roebling, upon learning this, assigned a man on horseback to accompany each wagonload, and in reporting the matter to the president of the Board stated (letter from the chief engineer to the president of Board of Trustees, New York and Brooklyn Bridge, dated July 9, 1878):

> "I owe it to myself to protect my reputation in this matter and in case a want of strength shall in the future be found in the cables, I wish the responsibility to rest where it belongs, with the Board of Trustees."

When asked to explain this statement, Colonel Roebling replied that the old Board of Trustees must be held responsible because,

> "They awarded so important a contract as the cable wire to a man who had no standing commercially or otherwise, and the same responsibility must be assumed by the present Board if they fail to at once put an end to [his] * * * contract, now that he has been detected in fraud. * * * I further recommend that you call for new bids for the wrapping wire as [his] * * * bid is below the cost of production and he must be intending to furnish something else than the material called for."

Colonel Roebling's bitterness was probably aggravated by a previous action of the Board of Trustees in voting to prohibit acceptance of bids from any firm or company in which any trustee, officer, or engineer of the bridge was interested, thus forcing him to sever all connection with the John A. Roebling's Sons Company. Although the Trustees did not cancel the cable wire contract, as urged by Colonel Roebling, they did reaward the contract for the wrapping wire.

It was estimated that about 221 tons of rejected wire had been spun into the cables. To compensate, 14.25 tons of wire, or, roughly, 1.7% was added to each cable. To have fully compensated for the estimated loss in strength would have required 22 tons of wire per cable, which would have made the cables too large to accommodate the cable bands.

Suspended Structure

The floor structure, as originally planned, consisted of six stiffening trusses of uniform depth when the floor beams framed at the neutral axis of the trusses. Between the outer pairs of trusses, provision was made for a sidewalk and a roadway for one lane of vehicles. An elevated promenade for the use of people of leisure and invalids was included between the inner pair of trusses in the plane of the top chords. The two intermediate spaces were designated for railway cars. To provide the additional underclearance required by the Secretary of War, the stiffening trusses were raised in relation to the floor beams, thus making it unnecessary to change the profile of the roadway. The

sidewalks were also eliminated, and the bridge was widened to provide roadways for two lanes of vehicles—a most fortunate decision, considering the needs of present-day motor traffic. At the same time, the inner trusses were made deeper to serve as railings for the promenade. The floor structure was to be constructed mainly of wrought iron. The design remained in this status until May, 1878, when, in spite of repeated objections by Colonel Roebling, the Board of Trustees voted that the bridge should be constructed to accommodate standard-gage "palace" or sleeping cars in order to make possible continuous transit between the two cities without transfer to bridge cars. Colonel Roebling had previously determined that provision should be made for a lighter weight passenger car with not less than 6-ft gages to insure steadiness in high winds.

The Board of Trustees believed its action would not cause a serious change in the plans (which had been completed); and, accordingly, bids for furnishing the ironwork were taken in June, 1878. The president was authorized by the Board to award the contract; but the cash on hand at the time was only $2,582.06, and, with liabilities of more than $157,000, no award was made. In the meantime, Colonel Roebling investigated the possibility of using steel and reported that (letter from the chief engineer to the president of Board of Trustees, New York and Brooklyn Bridge, dated April 2, 1879):

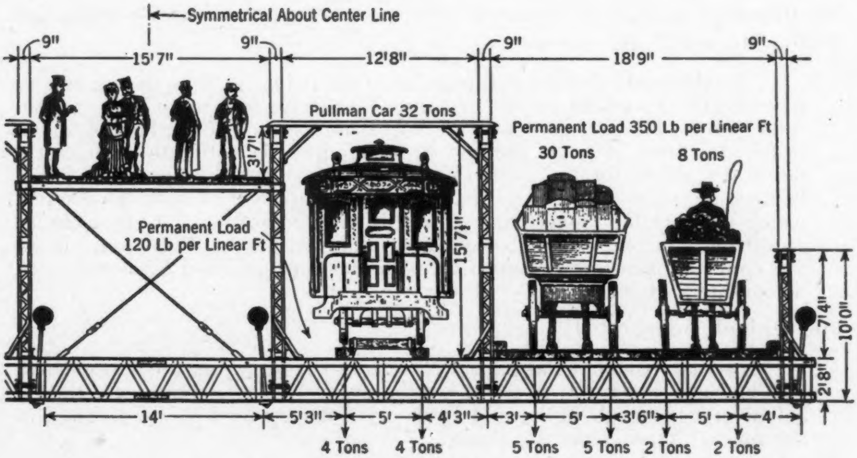
> "There will be nothing experimental, therefore, now on our part in the use of steel, as manufacturers have proved satisfactorily that a mild grade of steel in every way suited to bridge construction can be produced."

In April, 1879, the Board ordered that revised plans and specifications be prepared on the basis of using principally steel in place of wrought iron. Bids on this basis were to have been received on May 1 but were delayed until May 27, pending the outcome of an investigation of a charge against the engineers that they released advance information of the steel specifications for a consideration of $10,000. A committee of Trustees, after investigating, stated: "It is unnecessary to say that there is not the slightest evidence to sustain the charge."

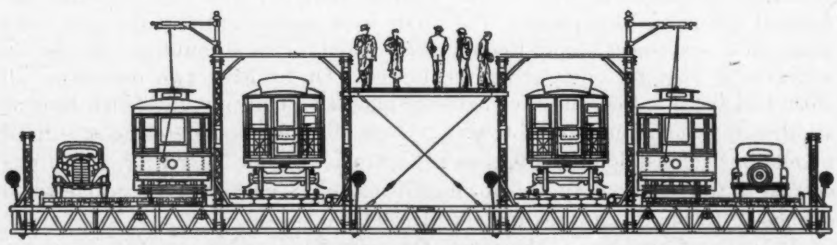
The contract was finally consummated on August 6, 1879, with the Edge Moor Iron Company, calling for the delivery of not more than 500 tons of steel during the remainder of that year. For financial reasons, no deliveries were to be made between January 1 and May 1, 1880, and the Board reserved the right, until May 1, to order the remainder of the steel called for in the contract. Progress was slow. Revised drawings were not completed until January, 1880. The unprecedented size of the channel sections made it necessary for the mill to design and construct new shears to cut the blooms and new rolls to form the channels. The engineers had also assumed that it would be possible to fabricate the eyebar stiffening-truss diagonals by the same procedure that had been used to make iron eyebars; but, when made in this way, the diagonals would not meet the specifications. Four months of constant and costly experimentation were required to develop a satisfactory method. It was not surprising, therefore, that the contract price increased from 4.35¢ a lb to 8.50¢ a lb for subsequent orders of steel not covered by the original contract. The

first shipment was made in November, 1880, and field erection was begun in January, 1881.

The original contract called for approximately 5,500 tons of steel. When the change from iron to steel was made, it was estimated that there would be a saving in tonnage of from 12% to 15%. As the work progressed, it was discovered that some additional 1,100 tons would be required. This was due partly to an increase in the depth of the four inner trusses (see Fig. 5(*a*)) and other strength-



(*a*) 1883–1897



(*b*) After 1897 (Trolley Cars Rerouted over Railway Tracks, December 15, 1944)

Fig. 5.—Cross Section at Middle of River Span

ening necessary to accommodate the Pullman cars, but largely because the steel could not be rolled as thin as iron. Considerable concern was expressed by the Trustees regarding the resulting margin of safety of the bridge, to which Colonel Roebling replied: "The margin of safety in this bridge is still over four, which I consider safe." He added, however, that perhaps an error of judgment had been committed in using a design stress of 15,000 lb per sq in. in place of 20,000 lb per sq in., although, even at the lower unit stress, some of the sections were deliberately made thicker to compensate for corrosion, which he probably visualized would result from "neglect of posterity."

Financial and Political Difficulties

Engineering and construction problems were not the only difficulties that confronted Colonel Roebling and his associates. In the preparation of the original design of the bridge, his father had estimated the cost of the structure at $7,000,000. This estimate, intentionally, did not make any allowance for real estate. John Roebling visualized approach structures supported on small masonry pillars and iron columns in such a way that the property beneath could be utilized for commercial purposes such as a market hall to supersede the "old Fulton Market." He stated:[2]

> "It is desirable that the construction of the Bridge and the improvements underneath should be carried on simultaneously, and upon one and the same plan. The Bridge floor will thus form the roof of the houses underneath and must be made fire and water-proof. My estimate includes the necessary pillars for the support of the Bridge floor, but no more. The balance of the work must be charged to the improvements of the ground, which will pay for themselves. For this same reason have I not included an estimate of real estate, because its value will not be destroyed, but on the contrary it will, in connection with the contemplated improvements, be considerably enhanced."

Furthermore, he reported:

> "It so happens that this part of New York has been very much neglected. The blocks are densely crowded by the poorest class of buildings, the removal of which will be desired by every citizen who feels an interest in the general improvement of the city."

Real estate costs, however, eventually totaled about $3,800,000. As construction advanced, it soon became evident that the cost was exceeding the original estimate. Frequently, the funds were almost entirely depleted, and upon such occasions Colonel Roebling was asked for an accounting and also an estimate of cost for completing the bridge. On at least two occasions, all work had to be halted and the engineers placed on half pay until such time as additional funds could be obtained. Once this required entering a suit of mandamus against John Kelly, then Comptroller of New York City, which was carried to the Court of Appeals for final decision. The cost of the bridge when opened to traffic was approximately $15,500,000.

It was not surprising, therefore, that suspicions were aroused as to the proper expenditure of the funds, and the records of the day are filled with insinuations and accusations which quite often led to investigations.

As organized by the Act of 1867 (Chapter 399), the New York Bridge Company was a private corporation with an authorized capital stock of $5,000,000. Of this sum, $3,000,000 was subscribed by the City of Brooklyn, $1,500,000 by the City of New York, and $500,000 by individual investors. The act was amended in 1869 (Chapter 26) to provide for representation of the cities on the Board of Directors; but, as these directors had no vote in the election of others, the control of the Board remained entirely in the hands of the private stockholders. Decisions on all business matters were made by an Executive Committee, it being ordered by the president of the company at the very beginning that: "* * * the Chief Engineer should have nothing to

do with the making of any contracts or purchase of any supplies." The action of this committee in appointing the majority stockholder as "general superintendent" on a 15% fee basis (including the cost of real estate) for supervising the construction of the tower foundations to high-water level, was, for a time, the subject of much bitter debate and an investigation which resulted in a considerable downward adjustment of the fee finally paid.

In June, 1874, the New York State Legislature again amended the Act of 1867 to provide for the purchase of the stock held by the private investors and to transfer the completion of the bridge to the direction of the mayors and comptrollers of each city, together with a Board of Directors appointed by them. The reorganization on this basis was modified by still another Act of Legislature passed in May, 1875 (Chapter 300), which provided for the dissolution of the New York Bridge Company and the establishment of the bridge as a public work of the two cities. The first meeting of the Board of Trustees appointed under authority of the act was held on June 9, 1875. This change seemed only to add to the suspicions of the general public. In 1876 an application for an injunction to halt further construction was made to the Circuit Court of the United States; and again, in 1879, upon petition of D. Willis James and others stating that the bridge would be unsafe as well as an obstruction to traffic, that it would reduce property values, and, furthermore, that it would cost in excess of the cost fixed by law, a complete investigation of the project from the very beginning was conducted by the Committee on Commerce and Navigation of the New York State Assembly. In the fall of 1882, another investigation of the affairs of the bridge was initiated by the Board of Trustees, following charges made by a daily newspaper, the *New York World*, accusing the Trustees of malfeasance in office, neglect of duty, conspiracy, and misappropriation of funds. All that this investigation disclosed, however, after more than a year of search, was an overpayment of $9,578.67 to some of the material contractors because of clerical errors.

Then, there were problems, such as the case of widow Delaney, whose husband had the misfortune to fall off the bridge. The Trustees paid the funeral expenses, amounting to $51.12 and already donated the sum of $25 to Mrs. Delaney; but she sought further assistance. It was decided that, although she had no legal claim on the Trustees and any donation granted her would be simply an act of charity, a sum not to exceed $100 was to be paid to her in instalments, at the discretion of the vice-president.

Opening Ceremonies

The bridge was finally completed and opened to the public on May 24, 1883. The opening ceremonies were of national importance, being attended by the President of the United States, the governors of several states, and the mayors of nearly all the cities in the vicinity.

It was a gala holiday and excursions were run by the railroads from the neighboring cities and towns. As President Chester A. Arthur, Governor Grover Cleveland, and Mayor Franklin Edson of New York City, walked over the bridge, escorted by regiments of the National Guard, salutes were fired from the Navy Yard and from the forts in the harbor, joined by five naval ships

anchored below the bridge. Whistles were blown and the chimes of Trinity Church in New York City rang out.

After the exercises, which were held on the Brooklyn side, the President, the Governor, and the Trustees were driven to the near-by home of Colonel Roebling to offer him their felicitations as he was unable even to attend the ceremonies. In the evening, the bridge was cleared for an elaborate display of fireworks.

The joys of opening day, however, were dampened a week later on Memorial Day when a skeptic yelling that the bridge was falling created a panic and caused the death of thirteen people on the promenade stairway at the New York anchorage.

Traffic

After considerable discussion and voting, the following toll rates were established by the Board of Trustees:

Traffic	Cents
Pedestrians	1
Railroad fare	5
One horse or horse and man	5
One horse vehicle	10
Two horse vehicle	20
Additional horses, each	5
Sheep and hogs, each	2
Neat cattle, each	5

In later years these rates were modified from time to time and, except for the railroad fare, were finally abolished in 1911.

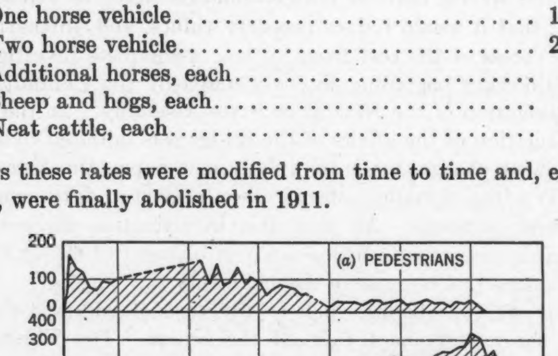


Fig. 6.—Record of Daily Traffic

Pedestrian traffic was particularly heavy at first and on Sundays and holidays one of the roadways was usually closed to vehicle traffic and made available for pedestrians to Brooklyn, the promenade being restricted to those crossing to New York. In the first year pedestrian traffic averaged about 16,600 daily (see Fig. 6) with morning and evening hourly peaks of more than

2,000. The fad of walking over the bridge apparently ended about 1910, because since then traffic has gradually decreased to about 3,000 people a day. By order of the Navy Department, the bridge was closed to pedestrian traffic on July 27, 1942, for the duration of World War II.

Railway traffic over the bridge began on September 24, 1883. This facility probably received more thought and planning than any other feature. John A. Roebling visualized multiple-car, rope-driven trains running alternately back and forth over each track. In 1878 Colonel Roebling still recommended this system, making the following pertinent comment in regard to the circulating system (letter from the chief engineer to the president of Board of Trustees, New York and Brooklyn Bridge, dated February 25, 1878):

> "Imagine for a moment that at 6 o'clock P.M., a single car on the circulating system with a carrying capacity of fifty passengers has arrived. A crowd of two hundred is waiting to get in! Who will enter it? Why the strong men and active boys. In a minute-and-a-half or probably two minutes, another car arrives. By this time a fresh relay of strong men and active boys is at hand, and the performance is repeated. It may thus happen that ladies and timid persons will have to wait a whole hour before they can cross."

It was finally suggested, however, to use circulating multiple-car trains stopping at elevated platforms on each end of the bridge and, on this basis, Colonel Roebling agreed that the circulating system was preferable and recommended its adoption. He also proposed the installation of tracks on the roadways for horsecar transportation, but this facility was later abandoned by action of the Board of Trustees.

Traffic using the rapid transit trains increased from approximately 25,000 passengers a day in 1883 to a maximum of about 266,000 a day in 1907, with a peak hour count of more than 46,000—nearly 82% more than the most optimistic forecast of such traffic made in 1879. Early in 1908, the first subway to Brooklyn was placed in operation and two years later the Manhattan Bridge was opened to traffic. Passenger traffic over the Brooklyn Bridge, accordingly, went on the decline and on March 5, 1944, when the service was discontinued, there were only about 7,800 passengers using the trains each day.

Single cars were used at the beginning, increasing in number as the traffic demanded. Steam locomotives were used to switch the cars at either end and to haul them over the bridge during the slack hours. On foggy days, flagmen were stationed at frequent intervals along the tracks to guard against rear-end collisions, until a satisfactory automatic block signal system became available in 1908. Oil lamps in the cars were replaced in 1895 with electric lights served by overhead trolleys. The locomotives were replaced by electric motor-driven cars in 1897, but use of the rope or cable power continued until 1908. The trains increased in length until six-car trains operating on a 1-min headway were used during the peak hours. Through trains from Brooklyn started running over the bridge in 1898 (the year that New York and Brooklyn were consolidated and the bridge was placed under the jurisdiction of the Department of Bridges of the City of New York).

By 1901 traffic facilities at the Manhattan terminal had become so overtaxed that the condition was brought to the attention of the New York County

Grand Jury by the District Attorney. During the next decade continual effort was made to alleviate the congestion. Legislation was passed and numerous plans were projected for a new station. Elevated lines to disperse the traffic to several stations were proposed and lines connecting with the Williamsburg and Manhattan bridges, then under construction, were also urged; but, except for acquiring property for a new station, nothing was ever accomplished because of the stolid inaction on the part of the city officials in the face of certain injunctions to any construction which might have been attempted. However, after repeated applications, approval was granted to the extension of the head house first to the west side of Park Row and later to the west side of Center Street (see Fig. 1), thus providing for six-car trains starting in January, 1908. This construction was regarded as temporary until a new station could be built but it remained in place until 1935.

The property acquired for the station was finally used as a site for the Municipal Building. In one last effort to connect the rapid transit tracks with those of the other two bridges, connections were constructed from the bridge approach passing under Park Row to a subway station beneath the Municipal Building. At this point the tracks were intended to connect with those of the Centre Street subway which was opened to traffic in 1913. The construction of the connections, except for the installation of the tracks, was completed in 1914, but these connections were never used because the operating company refused to enter into an agreement with the city.

Tracks for trolley cars were installed on the bridge roadways in 1897 (see Fig. 5(*b*)) and within a few years the trolleys were transporting approximately 169,000 passengers a day. This traffic has also decreased to about 38,000 passengers a day. Work has been completed which on December 15, 1944 enabled the rerouting of the trolley cars over the tracks formerly used by the trains. The stations, which had long since become an eyesore as well as a maintenance headache, have also been demolished.

Vehicular traffic over the bridge increased, roughly, from 1,700 vehicles a day in 1883 to approximately 30,000 a day in 1940. This seems to be about the limit of capacity, however, when compared with the traffic trends of the other East River bridges.

Pneumatic mail-tube service over the bridge was initiated in August, 1898, reducing the time of transporting mail between Manhattan and Brooklyn from 25 min to 3 min.

Interruptions of Traffic

During its existence, the Brooklyn Bridge has not always operated without interruption. On July 29, 1898, a horse died on the south roadway near the Brooklyn tower, completely blocking all Brooklyn-bound vehicular and trolley traffic. The entire river span was soon jammed, causing a shifting of the cable saddles riverward and a readjustment of stresses in the overfloor stays. Some of these stays, being attached directly to the tower tops rather than to the saddles, picked up more than their share of the load, thus causing a severe lateral distortion of the stiffening trusses described in the records as a "buckling" of the bottom chords. No mention was recorded of what repairs,

if any, were necessary; nor is there any physical evidence of any having been made. The condition partly corrected itself with the dispersion of the live load and the saddles slowly returned to their former position.

Again, on July 24, 1901, nine consecutive suspenders at the center of the river span along the north cable were found broken, requiring the cessation of all rail traffic for 36 hours. These suspenders are short, solid, round rods connected to the floor system through trunnions to permit longitudinal movement. The discovery attracted the attention of Eugene Philbin, then District Attorney for New York County who retained Edwin Duryea, Jr., and Joseph Mayer, Members, ASCE, consulting engineers, to

> "* * * make an examination of the bridge * * * with a view of ascertaining whether or not the capacity of the bridge is overtaxed * * * and as to whether or not the bridge has been allowed to deteriorate because of improper supervision, inspection and repair."

These engineers submitted a lengthy report severely criticizing the men entrusted with the care of the bridge and also certain features of the design of the structure. A careful review of the report made by Richard S. Buck, M. ASCE, chief engineer and Henry A. LaChicotte, assistant engineer of the Manhattan and Queensborough bridges at the request of Charles C. Martin, M. ASCE, chief engineer of the Brooklyn Bridge, not only refuted all accusations of lax supervision, inspection, and maintenance, but also showed basic fallacies in the Duryea-Mayer analysis of the design. Wilhelm Hildenbrand M. ASCE, who, with Mr. Martin, had been associated with Colonel Roebling on the design and construction of the bridge also refuted the Duryea-Mayer report. No official action was taken by the District Attorney.

Motor vehicles were barred from the bridge on July 6, 1922, following a riverward shifting of the northerly two cable saddles over the Manhattan tower. From a study of saddle movements, it appears that these movements were simply the readjustment of cable stresses consistent with the condition existing in the other two cables. The ban against motor vehicles was lifted to passenger cars on May 12, 1925.

The Eighth Wonder of the World

The completion of the Brooklyn Bridge was without question a milestone in the science of suspension bridge construction. Its span held the record for 20 years and was not materially exceeded until 1925, when the Philadelphia (Pa.)-Camden (N. J.) Bridge across the Delaware River was opened to traffic. It also marked an advance of 51% in length of span which was not surpassed until the completion of the George Washington Bridge across the Hudson River between New York City and Fort Lee, N. J., in 1931, which increased span length 89%.

Considering the manifold unprecedented problems which confronted its builders, and the limited means with which to solve them, the Brooklyn Bridge truly stands as, so frequently described, "The Eighth Wonder of the World."

References

Many references were consulted in the preparation of the Symposium, some of them probably available only in the files of the Department of Public Works of the City of New York. An exceedingly comprehensive bibliography on the Brooklyn Bridge may be found in **"A History of Suspension Bridges in Bibliographical Form,"** by A. A. Jakkula, U. S. Public Roads Administration and the Agricultural and Mechanical College of Texas, College Station, 1941, pp. 198–236. The following references proved to be the most productive of useful information:

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DESIGN AND STRESS CONDITION

BY O. H. AMMANN,[8] M. ASCE

INTRODUCTION

The Brooklyn Bridge is a structure of momentous interest in the history of bridge building. The bridge furnishes a wealth of information on the safety and useful life of a structure of its kind and on the merits and shortcomings of its design, as viewed from present-day experience and advanced knowledge.

Its construction was the culmination of a series of splendid records in suspension bridge building in the nineteenth century. Remarkable progress in suspension bridge building began with the achievements of English and French engineers in the early part of that century. In 1826 Thomas Telford completed the famous Menai Bridge in Wales with the then unprecedented span of 576 ft; and, in 1834 the French engineer, M. Chaley, built an 870-ft span at Fribourg, Switzerland.

In the second half of the nineteenth century attention was focused on the United States because of new achievements in the field of suspension bridges and advances in the art of building wire cables to new records of span. John A. Roebling, who planned and initiated the building of the Brooklyn Bridge, himself had created a number of outstanding bridges of this type, among them the unique Railway Suspension Bridge across the Niagara River in 1855 and the bridge across the Ohio River at Cincinnati in 1867. With its span of 1,057 ft, the Ohio Bridge was at that time the longest suspension bridge and served as a close model for the Brooklyn Bridge.

The latter, however, outranked its predecessor because of a 50% longer span (1,595 ft), exceptionally long side spans (933 ft), and more than 100% greater carrying capacity. The Brooklyn Bridge is also distinguished by the first introduction of steel wire in place of the wrought-iron wire previously used for cables. Cable wire was increased in strength from 90 kips per sq in. to 160 kips per sq in., and thus much longer and heavier spans were possible. For the first time, also, the cable wire received a galvanized zinc coating, instead of a film of grease, which contributed to increased life.

Together with the Eads Bridge in St. Louis, which was under construction at the same time, the Brooklyn Bridge has the distinction of being one of the first large bridges in which pneumatic caissons were used in building great foundations.

CONCEPTION OF THE ORIGINAL DESIGN

Although the design of the Brooklyn Bridge reflects general practice in the nineteenth century, in many respects it reflects the individual conceptions of John A. Roebling. The most conspicuous features are the massive masonry towers, which were common in early suspension bridges (Fig. 7). Mr. Roebling properly remarked that "in a work of such magnitude, located between two

[8] Cons. Engr., New York, N. Y.

great cities, good architectural proportions should be observed." He stated:[9]

> "The great towers will serve as landmarks to the adjoining cities, and they will be entitled to be ranked as national monuments. As a great work of art, and as a successful specimen of advanced bridge engineering, the Brooklyn Bridge will forever testify to the energy, enterprise and wealth of that community which shall secure its erection."

The graceful cable curve with its small sag (one thirteenth of the span, which is characteristic of older suspension bridges) also contributes to good

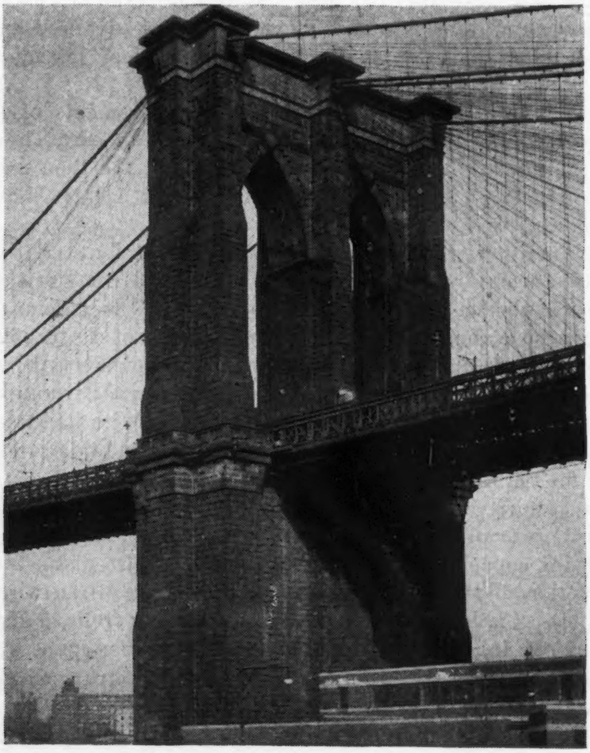


FIG. 7.—"THE MOST CONSPICUOUS FEATURES ARE THE MASSIVE MASONRY TOWERS'

appearance. The relatively shallow parallel-chord stiffening trusses along the floor, although considerably deeper than those originally conceived by Mr. Roebling, do not detract from the elegance of the structure. The inclined stay ropes add to the unique appearance.

At the time of the conception of the Brooklyn Bridge, suspension bridge building had already been highly developed as an art, but proportioning had not yet reached the stage of refined modern science. The theory of the suspended

[9] "Report of John A. Roebling, C. E., to the President and Directors of the New York Bridge Company on the Proposed East River Bridge," Eagle Book and Job Printing Dept., Brooklyn, N. Y., 1867, p. 3

cable was well known, as was also the common theory of elastic structures; but the designers of the Brooklyn Bridge were not familiar with more refined methods which would have aided in the analysis of the highly indeterminate structure. The so-called deflection method of calculating stresses, which is now commonly applied to suspension bridges, was developed only in the early years of the twentieth century. Whatever records remain of the stress calculations used in designing the Brooklyn Bridge indicate a very crude conception and analysis of the stress action in the stiffening system.

Nevertheless, Mr. Roebling had a keen practical understanding of the suspension system and the functions of the component parts. He appreciated the merits inherent in the simple unstiffened cable suspended from the towers. Experience had amply demonstrated that the cable had to be stiffened against objectionable or destructive oscillatory motions incited by moving loads or wind; but Mr. Roebling realized that such stiffening must be kept to a rational limit. Contrary to this wise view, in the early part of the twentieth century, there was a fallacious tendency to make a virtue of rigidity, which resulted in many excessively rigid, economically wasteful, and clumsy looking suspension bridges.

Previous developments had led to two essentially different methods of stiffening, one by a system of inclined stay ropes and the other by stiffening girders or trusses along the floor. Mr. Roebling believed that he could obtain greatest effectiveness by a combination of the two. He assigned to the trusses the principal task of stiffening the cables vertically in the center of the center span and in the anchorage portion of the side spans, where the stays cannot be effective. He also considered the trusses essential in resisting dynamic wind action, for he stated:[10]

> "* * * to guard against vertical and horizontal oscillations, and to insure that degree of stiffness in the flooring which is absolutely necessary to meet the effects of violent gales in such an exposed situation, I have provided six lines of iron trusses, which run the whole length of the suspended floor from anchor wall to anchor wall * * *. Those parts of the trusses which extend below the floorbeams, afford an excellent means for lateral trussing and bracing."

According to Mr. Roebling's original plans, all six trusses were to be 12 ft deep, or only one one hundred and thirtieth of the center span. For the traffic then contemplated these flexible trusses would have been adequate, but (see Mr. Seely's paper) it appears that, during construction of the bridge, Col. Washington A. Roebling (who became chief engineer after his father's death) was induced by the directors of the Bridge Company to provide for the possibility that heavy Pullman cars might eventually be moved over the bridge. This called for stiffer trusses near the tracks and thus the four inner trusses were increased in height to 17 ft.

John A. Roebling assigned to the stay system the principal task of reducing the motions of the cable saddles under unbalanced loads on center and side spans. The stays, in combination with the stiffening trusses, fixed at the

[10] "Report of John A. Roebling. C. E., to the President and Directors of the New York Bridge Company on the Proposed East River Bridge," Eagle Book and Job Printing Dept., Brooklyn. N. Y., 1867, p. 17.

towers, form a cantilever system that relieves the cables of a substantial part of the vertical load. This stay system is so strong (John A. Roebling stated), that:[11]

> "The supporting power of the stays alone will be ample to hold up the floor. If the cables were removed, the Bridge would sink in the center, but would not fall."

Col. Washington A. Roebling disclosed that he was aware that the stays do not act in perfect harmony with the cables and that for a single span he would not advocate their use; yet in this case, on account of interaction between center span and the long side spans, he considered them indispensable and he maintained that the "want of harmony between stays and cables is less in reality than in theory."

It is not clear from the records why unusually long side spans were adopted —very likely to reduce the height of the anchorages and thus to minimize their cost. John A. Roebling realized the resulting difficulties, especially in maintaining "the balance of spans," as he called it. Originally, he considered anchoring down the cables of the side spans halfway between the towers and the anchorages—an idea which was advanced in subsequent years by several engineers who reviewed the design. Mr. Roebling abandoned this feature, however, as not being very effective, and he decided that the motions of the cable saddles were most effectively checked by the stay system.

The bridge was originally built to accommodate (on two tracks) passenger trains composed of cars attached to an endless rope propelled by a stationary engine. Two 2-lane roadways were to serve horse-drawn vehicles, and a pedestrian promenade was provided on a central elevated platform (see Fig. 5).

The live load corresponding to this traffic capacity was estimated at 1,800 lb per ft of bridge and, with an estimated dead load of 8,750 lb, brought the total load to be carried by the bridge to 10,550 lb.

In course of time a trolley track was placed on each roadway, the inner tracks were used by heavier electric trains, and certain additions were made to the floor structure, so that for a period of time the bridge carried as much as 40% more load than was originally intended.

Review of Design in Light of Present Knowledge

In 1943, on the eve of transforming the Brooklyn Bridge from a structure principally for passenger traffic in electric trains and trolley cars into a structure for modern highway traffic, it appeared advisable to review its design with a complete calculation of the stresses and to undertake an examination of its physical condition.

The completed investigation aimed first at determining the limitations of capacity without major alterations and second at exploring the possibilities of major reconstruction to create an up-to-date link in the highway system.

The Main Carrying System: Cables, Stiffening Trusses, and Stays.—Study of the original design and changes made in course of time reveal a stress condition

11 "Report of John A. Roebling, C. E., to the President and Directors of the New York Bridge Company on the Proposed East River Bridge," Eagle Book and Job Printing Dept., Brooklyn, N. Y., 1867, pp 18 and 19.

that is far more complex, statically indeterminate, and uncertain than would appear from a superficial examination of the geometry of the system.

Fortunately, however, the uncertainty of stress action does not apply to any great extent to the carrying members upon which the safety of the structure depends—the cables, towers, and anchorages. Uncertainty as to stress action is confined to the vertical and lateral stiffening systems, damage to which does not necessarily endanger the safety of the structure.

As in most suspension bridges the four cables are continuous from anchorage to anchorage and form natural catenaries under the loads transmitted to them by the suspenders (Fig. 8). On top of the towers the cables rest on cast-iron

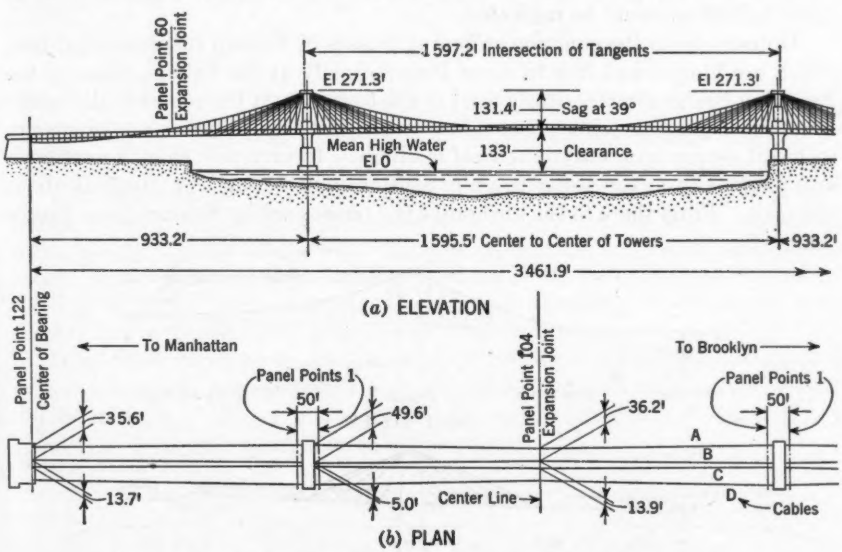


FIG. 8.—GENERAL PLAN AND ELEVATION, THE BROOKLYN BRIDGE

saddles and these in turn bear on nests of rollers designed to permit longitudinal motions of the cables under temperature changes and unbalanced loads. These rollers are very small (only 3.5 in. in diameter); and in course of time collection of dirt and rust, and in some cases dislocation of the rollers, increased the friction to such an extent that under most ordinary conditions no motion took place. Only on a few rare occasions of severe loadings was a "slipping" of the saddles observed. For future highway traffic, the existing roller nests may be considered frozen, and any unequal cable pull can be assumed transmitted to the rigid towers.

The bridge thus acts as three independent suspension spans, and this condition was assumed for the calculation of stresses in the first phase of conversion, in which the cable saddles would be left as they were. This realistic assumption simplified the stress calculations. On the other hand, the inaction of the roller nests has introduced uncertain stresses because it is not known under what temperature and live loads the rollers did become frozen. The floor is

suspended from the cables by the customary wire-rope suspenders at the floor beams.

Six stiffening trusses are placed along the floor (see Fig. 5); four of them are close to the four cables and the two intermediate trusses are placed between the outer and inner cables. The four inner trusses were made 17 ft deep, whereas the outer trusses are only 8.75 ft deep. This cross-sectional arrangement is one source of indeterminateness, although not a major one since the continuous and closely spaced floor beams are sufficiently rigid to assure a fairly even deformation of the six trusses. Furthermore, the moment of inertia of the two outer trusses is only about one eighth of that of the four inner ones, so that their effect could be neglected.

Differing from the common stiffening trusses of modern suspension bridges, which are hinged and free to move longitudinally at the towers, those of the Brooklyn Bridge are practically fixed at the towers. At the center of the center span, an expansion joint is provided which, although it acts as a hinge can transmit shears from unsymmetrical loadings. In each side span, a cantilever arm extends from the tower to an expansion joint acting as a hinge at about midspan. From there to the anchorage the trusses act by themselves as simple spans (Fig. 9).

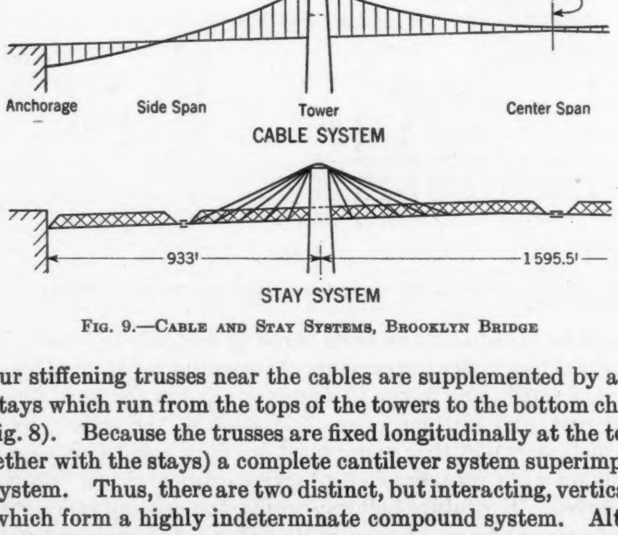


FIG. 9.—CABLE AND STAY SYSTEMS, BROOKLYN BRIDGE

The four stiffening trusses near the cables are supplemented by a system of diagonal stays which run from the tops of the towers to the bottom chords of the trusses (Fig. 8). Because the trusses are fixed longitudinally at the towers they form (together with the stays) a complete cantilever system superimposed upon the cable system. Thus, there are two distinct, but interacting, vertical carrying systems, which form a highly indeterminate compound system. Although the interaction between the two systems for temperature, live load, and any additional dead load can be calculated fairly reliably on the basis of the elastic properties of the component members, the stresses under the original dead load are by no means determinable with any degree of accuracy.

The lower chords of the stiffening trusses must resist the horizontal components of the stay tensions in addition to the flexural stresses caused by the bending of the trusses. These direct stresses become cumulative and very large toward the towers and have been the reason for overstress in the bottom chords.

The web system of the four deep stiffening trusses is formed by vertical posts at the floor beams and a quadruple system of diagonals composed of forged bars (Fig. 10). Half the diagonals (the so-called "counters") are adjustable in length. In course of time many of the counters have been tightened far beyond a nominal initial stress, thus inducing appreciable uncertain stresses.

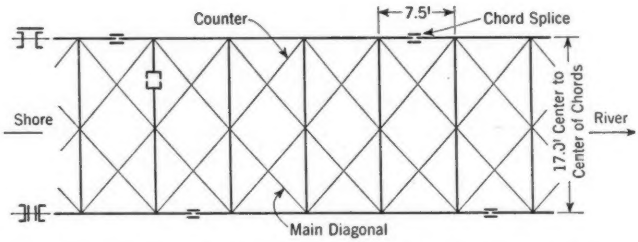


Fig. 10.—Inner and Intermediate (High) Stiffening Trusses

The top chords of the trusses were provided with a number of construction joints, three in each half of the center span, which created zero moments at those points. These joints were later bolted tightly; but, since there is no record of the stress condition existing at the time of the bolting or of the final riveting of other truss connections, another uncertainty is introduced with respect to the stresses from the original dead load.

In addition to these complications and uncertainties affecting the primary or axial stresses, secondary stresses are caused by faulty or crude design details or lack of proper maintenance. In fact, such defects have led to a number of local structural failures in the past.

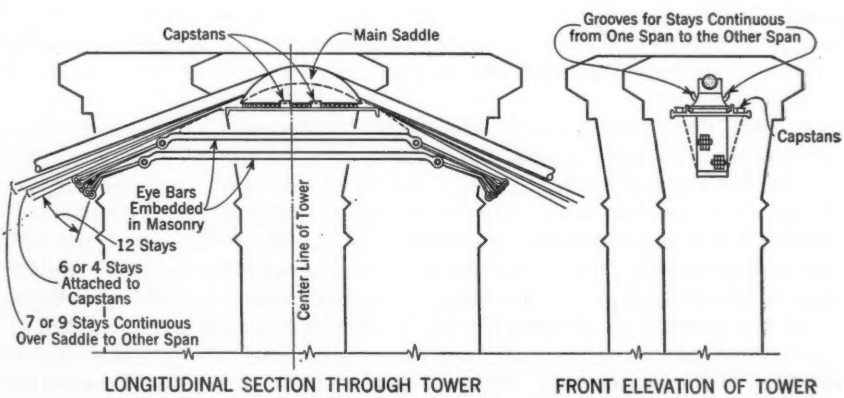


Fig. 11.—Arrangement of Overfloor Stays at the Tower Tops

One of the most incongruous details is the attachment of the stay ropes at the tops of the towers (Fig. 11). The stay ropes nearest the towers are connected to wrought-iron bars anchored in the tower masonry. Others are fixed to the saddle bed plates and some pass over the saddles. As a result, the latter group moved longitudinally with the saddles, while the others remained fixed.

Such motions must at times have caused appreciable redistribution of the stresses between the various stays and between the stays and the main cables. It would seem that on one occasion excessive strain put on certain stays caused excessive compression, with resulting bending or initial buckling, in the bottom chords of the stiffening trusses to which the stays are attached.

Under these conditions an accurate stress calculation throughout the structure is impossible. However, by the approximate analysis adopted, the stress condition has been determined with a sufficient degree of reliability to assure safety of the structure under actual load conditions.

Determination of Loads.—The present dead load was determined from the available records, supplemented by field measurements. The field survey showed a fairly uniform present dead load of 9,200 lb per lin ft or 800 lb per lin ft more than the weight when the bridge was completed. This increase is due largely to changes made in the deck structure.

When an estimate of the live load was being made, it was known that the elevated trains would be abandoned. They have since been removed, permitting the use of the tracks between the inner stiffening trusses by the trolley cars and leaving the two 17-ft roadways free for vehicular traffic.

Preliminary calculations indicated that a live load corresponding to unrestricted vehicular traffic on the two roadways would cause excessive stresses; and for this reason, as well as on account of the narrowness of the roadways, it was concluded that the vehicular traffic should be restricted to passenger automobiles as, in fact, it has been since 1925. On this assumption, the live load used for the calculations of the stresses in the existing structure was estimated at 2,400 lb per ft of bridge.

When applying this live load in the design of the stiffening system, it was realized that, imposing maximum load on partial stretches, with no load on the remainder of the span, would cause an excessively severe condition. Therefore, a light load of 600 lb per ft was assumed on the intervening stretches. The live load was combined with a maximum variation of temperature of $\pm$ 50° F.

Method of Stress Analysis in Main Carrying System.—The stresses in the vertical carrying system were determined by a combination of analytical approximations and measurement of certain stresses and deflections on the bridge. This procedure was ably devised by the staff of the Department of Public Works under the general guidance of Mr. Seely (author of the first Symposium paper). Mr. Delson developed and directed the theoretical analysis; and Mr. Gray (author of the third Symposium paper), the field operations.

It was essential to establish the exact cable curve under dead load and at known temperature, since it deviates considerably from the parabola due to the effect of the stay system. This was done by measuring the cable ordinates with reference to a carefully surveyed profile of the bridge floor. In the central section, outside the stay attachments to the floor, the cable curve was found to be a parabola, as expected; but it flattens considerably toward the towers, indicating the increasing absorption of the floor panel loads by the stay system. The maximum deviation from the parabola under dead load at 39° F is 4.75 ft in the center span and 5.3 ft in the side span (Fig. 12).

It was also essential to determine directly the dead-load tensions in the stay ropes. This was done by measuring the sag of certain selected stays and calculating the tension which produces that sag—proper adjustment being made for temperature differences. . .

From the dead-load concentrations and the stay tensions thus found, it was possible to determine the distribution of the dead load between cables and

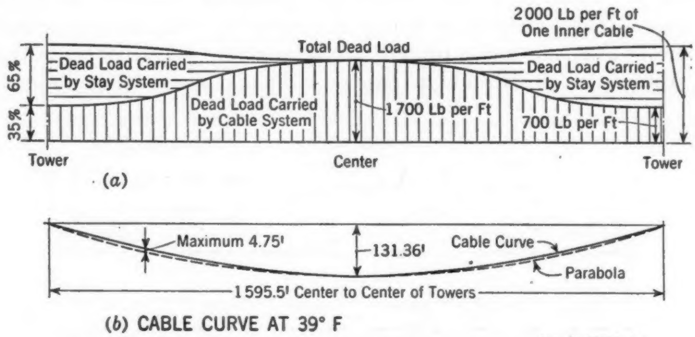


FIG. 12.—DISTRIBUTION OF DEAD LOAD IN THE CENTER SPAN

stays, to check the form of the cable curve, and to calculate the dead-load stresses in the cables.

Since there was no record of the erection procedure with respect to the stiffening trusses, it appeared reasonable to assume that, as is done in modern practice, the bolted construction joints in the top chords remained untightened and free to move and also that the other truss connections remained unriveted, until at least the greater part of the dead weight was in place. Under this assumption the trusses would be practically free from the original dead-load stresses. The stresses from subsequently added dead load were calculated by the method developed for the live load.

The stresses from live load and temperature were calculated first by a so-called elastic theory—that is, the relieving effect of the deflections was ignored. Since there are fifty stays in the center span, with twenty five near each end, in addition to the unknown cable pull, for any unsymmetrical live load, the elastic system would be statically indeterminate in the fifty-first degree. To simplify the laborious and unnecessarily refined procedure involved, the stays were assumed to act in groups of several adjacent stays of equal tension and each group was replaced by one resultant. Thus, the center-span system was simplified to a system statically indeterminate in the seventh degree for unsymmetrical loads. For symmetrical loads in the center span and for each side span the system became indeterminate in the fourth degree, requiring the solution, respectively, of seven and four elastic equations for each separate loading.

A further simplification was introduced by dividing the spans into approximately equal individual live-load lengths and assuming only full-load lengths for any load position. The moments and shears were calculated separately for

each load length and the results were combined for maximum values (Fig.13). The chord stresses so found were then reduced to allow for the effect of the deflections caused by the live load. The reduction was determined by measuring the deflections and certain chord stresses under an experimental elevated train placed on the bridge and then comparing the stresses with those calculated by the elastic theory for that loading. This procedure led to stress reductions of from 15% to 25% in the side span and from zero to 20% in the center span. These reductions compare reasonably with those calculated for a number of other suspension bridges, considering the contribution of the stays to the stiffness of the Brooklyn Bridge. Finally, the readily determinable

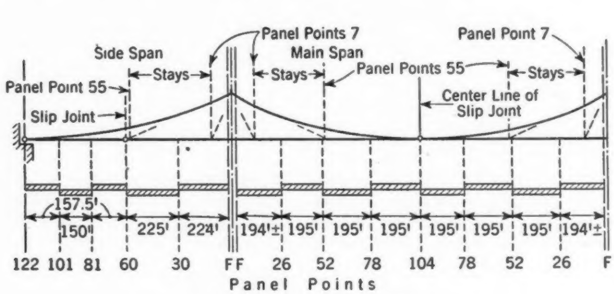


FIG. 13.—ASSUMED LOAD LENGTHS

direct compression stresses in the bottom chords were added to the flexural stresses, so reduced, as horizontal components of the stay-rope tensions.

The Make-Up and Strength of the Principal Members and Their Safety Under the Assumed Loads.—The stress investigations were supplemented by a program of tests to determine the present strength and other physical characteristics of the more important members, cables, anchorage chains, stay ropes, chords, and web members of the stiffening trusses. These tests are described more fully in the third and fourth papers of this Symposium.

Each of the four cables has a diameter of 15.75 in. and is composed of nineteen strands of wires which vary considerably in size, some being No. 7 gage and some No. 8 gage. The aggregate net area has been estimated at 144.8 sq in. Specimen tests on sixty wires cut from the cables show an ultimate strength ranging from 146,000 lb per sq in. to 192,000 lb per sq in., with an average of 162,600 lb per sq in. Other properties, although indicating a fairly hard steel, give assurance of an acceptable condition of the wire, with no apparent deterioration since it was erected.

The maximum axial stress in the cable, under the live load of 2,400 lb per sq in., is calculated at 44,600 lb per sq in.—less than 30% of the average strength of the wire. The physical qualities of the wire would justify a cable stress of 60,000 lb and therefore permit an increase in cable stress of as much as 33% in case of an eventual reconstruction of the floor structure. In such reconstruction, if the stay system is eliminated, thus throwing the entire load on the cables, the margin for additional load would be only about 18%.

The stays are steel wire ropes 1.75 in., 1.875 in., and 2 in. in diameter.

A full-size test of a rope disclosed an ultimate strength of 147,000 lb per sq in. The calculated live-load stress of 23,000 lb added to the dead-load stress of 21,000 lb derived from the sag measurements gives a maximum stress of 44,000 lb per sq in., which is only 30% of the ultimate strength of the rope. Therefore, the stays are also capable of sustaining a considerable increase in live load.

The stress calculations and measurements disclosed that the chords and web members of the stiffening trusses were the weakest parts of the structure. This is corroborated by the many web members that have broken and the chords that have been bent under severe load conditions.

A typical chord is composed of two 9-in. rolled channels, reinforced in the heavier sections by web plates. The top chords were originally also provided with a cover plate, and in later years cover plates were added to many bottom chords (Fig. 14). In place of efficient latticing, the two channels are connected

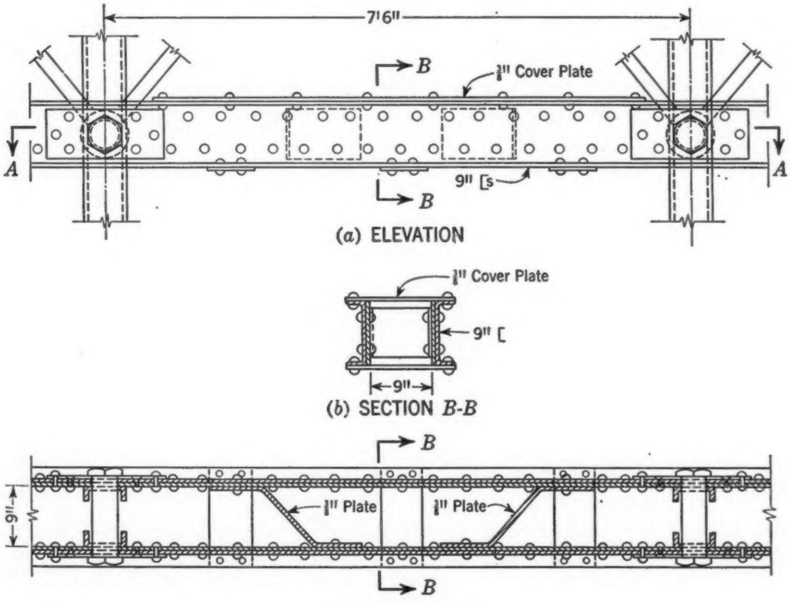


FIG. 14.—TYPICAL PANEL, BOTTOM CHORD

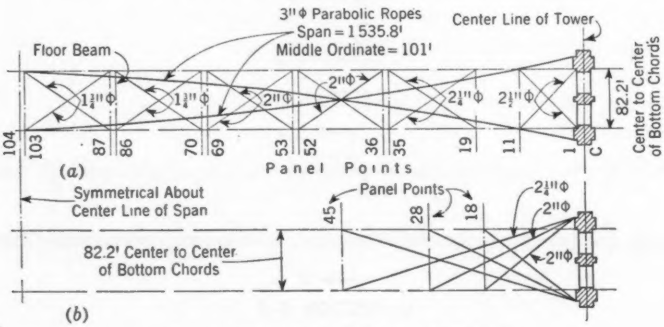
(in each 7.5-ft panel only) by two bent plate diaphragms. This very inefficient detail gave reason to expect that the chords have a relatively low buckling strength and it was deemed advisable to make a full-size model test. The model chord, built without cover plate and of ordinary structural steel, actually developed an ultimate strength of only 78% of the average yield point of the material.

The actual chords are made of high carbon steel. Specimen tests cut from the chords revealed an average yield point of 48,700 lb per sq in.—as high as

that for present-day silicon steel. Application of the 78% ratio to this yield point gave an estimated buckling strength of 38,800 lb per sq in. for the chord members without cover plates. With allowance for the cover plates, where added in later years, the strength was estimated at between 40,000 lb per sq in. and 42,000 lb per sq in. of the original section.

Compared to these strength figures, the calculations indicate compression stresses in some bottom chord members of more than 40,000 lb per sq in. and in a large number of more than 30,000 lb per sq in. In view of these large calculated stresses, it is not surprising that, in the region of the most highly stressed chord members, the latter sustained permanent deformations. On the other hand, although the chords were never straightened, no more serious consequences resulted thereafter, demonstrating that relatively high working stresses in the stiffening trusses of suspension bridges can be permitted. Nevertheless, it is advisable to reinforce the overstressed chord members in the Brooklyn Bridge.

All truss diagonals and counters are forged steel eyebars. A number of full-size bars, which were removed from the bridge and tested, revealed a yield point of from 43,000 lb per sq in. to 46,000 lb per sq in. and an ultimate strength of between 69,000 lb per sq in. and 76,000 lb per sq in. The initial dead-load stresses in the diagonals should be only nominal. Actually, because of evidently promiscuous tightening of the counters, these stresses in the counters, and consequently also in the main diagonals, were greatly increased. Stress measurements revealed actual dead-load stresses of more than 20,000 lb per sq in. That such stresses are easily produced is indicated by the fact that a single 360° turn of the adjusting sleeve causes a stress of 30,000 lb per sq in.



Note (a) and (b) Represent Ropes in Same Plane

FIG. 15.—PLAN OF UNDERFLOOR STAYS

The sum of the calculated live load and temperature stresses, which in some members are also nearly 20,000 lb, explains why under former severe load conditions not infrequent breakage of the diagonals occurred.

For the proposed restricted vehicular traffic it appears advisable to adjust all counters so as to reduce the dead-load stresses to a nominal minimum. The diagonals will then have enough spare strength for such traffic.

The Lateral System.—The system designed to resist wind and other lateral forces is also one of great complexity (Fig. 15). It is composed in part of a

lateral truss whose chords are the lower chords of the outer stiffening trusses. The web system is formed by the floor beams and double wire-rope diagonals. Additional diagonal ropes are connected directly to the towers. Thus, in each half of the center span, and between the tower and the hinge in the side span this lateral truss acts as a cantilever arm fixed at the towers.

The truss system is supplemented by a pair of parabolic wire-rope cables, 3 in. in diameter, which, in the center span, stretch from tower to tower with a central sag somewhat greater than the width of the floor. In spite of the relatively wide floor (one nineteenth of the main span), the lateral truss is very flexible on account of the extensibility of the wire-rope diagonals. The very flexible parabolic wind cables likewise offer little resistance to lateral deflection. Consequently, the floor deflects easily and transmits a substantial part of the wind force to the main cables, which thus become the third important element resisting the lateral forces. Fig. 16 shows the calculated distribution of the wind load between lateral truss, wind cables, and main cables. Area ABC in Fig. 16 represents the wind load transferred from the deck to the cables.

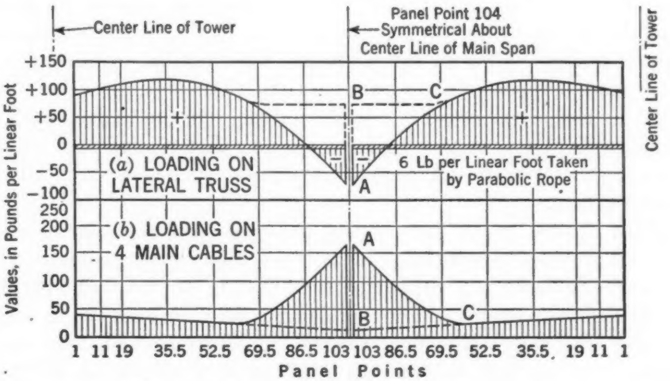


Fig. 16.—Wind-Load Distribution, Main Span

To enhance lateral resistance, the main cables were placed in inclined planes as in many earlier bridges. Today this cradling of the cables is recognized to be of negligible effect. The prevailing practice of proportioning the lateral system for a static wind force of 30 lb per sq ft of exposed surface (which corresponds to a wind velocity of about 100 miles per hr) would require the Brooklyn Bridge to be adequate for a wind force of about 950 lb per ft of bridge. This force would deflect the bridge laterally at the center 6.5 ft, or by one two hundred and fiftieths of the span, and would cause wind stresses of about 30,000 lb per sq in. in the truss chords. Such stresses in possible combination with the high live-load stresses must be considered excessive.

Such a condition, however, appeared unrealistic. Observations showed maximum deflections, under a quartering wind of 35 miles per hr, of only 2 in.—or a fraction of the theoretical deflection corresponding to this wind velocity. This great difference between actual and theoretical deflections must be due

in large part to the fact that the static wind pressures acting on large suspension bridges are far lower than the theoretical ones assumed in modern design. This is confirmed by observations on a number of other large suspension bridges.

On the basis of observed deflections it was concluded that under severest wind velocity the Brooklyn Bridge would probably not deflect more than 1 ft at the center. This assumption was made in computing the wind stresses, which were found to be very moderate, both in the trusses and in the parabolic wind cables. The latter are probably rarely under stress.

In view of the fact that the lateral flexibility of the Brooklyn Bridge is within the range of that of several longer and much narrower suspension bridges which have experienced oscillations in wind, it seems pertinent to inquire what resistance the Brooklyn Bridge offers to such motions, which were disastrous to the Tacoma Narrows Bridge in the State of Washington.

Contrary to widespread belief, evidence shows that within practical limits lateral flexibility has little or no influence on oscillations in wind. The behavior of many suspension bridges during the 120 years since about 1825 furnishes unmistakable evidence that rigidity of the carrying system in the vertical planes of the cables is the predominant factor in the resistance against aerodynamic motions. Based on this empirical information, expressed in rational analytical form, it is possible to evaluate existing bridges and to design new ones for optimum safety with respect to other forces. On the basis of such analysis, the Brooklyn Bridge is shown to have ample resistance against aerodynamic oscillations—during its life it has never experienced such motions or any ill effects under wind action.

The Floor System.—The floor system of the Brooklyn Bridge is very simple (Fig. 5). It consists essentially of trussed floor beams spaced at 7.5 ft. Originally wooden stringers supported the rails and the roadway planks rested directly on the floor beams. The wooden beams have since been replaced by shallow steel stringers. The floor beams are continuous on four elastic supports formed by the four cables. In addition to transmitting the floor loads to the cables, the floor beams must distribute the proper share in resisting bending to the intermediate stiffening trusses.

Under former severe traffic conditions, coupled with high secondary stresses due to faulty details, the floor beams must have been subjected to high stresses, and consequently suffered local fractures of the chords on several occasions. Under restricted vehicular traffic, even allowing for an occasional 10-ton truck, the stresses are well within permissible limits, so that, except for local corrections, strengthening of the floor beams is not necessary.

The Towers.—The towers below the floor are massive, but partly hollow masonry pillars, mostly of granite (Fig. 17). Above the floor they consist of three masonry shafts joined at the top by Gothic arches. The towers are 271 ft above high water, 140 ft long, and 59 ft wide at the water line. Through a spreading base, mostly of limestone, each tower rests on a concrete-filled timber caisson about 170 ft long and 102 ft wide. As previously mentioned, these caissons were sunk by the pneumatic process. They were launched in 1870 and 1871 and were then of unprecedented size.

The New York caisson rests, at a depth of 78 ft, on a shallow layer of hard compact material overlying the bedrock of gneiss. The Brooklyn caisson is founded on a compact formation of sand, gravel, and boulders at a depth of 45 ft.

Because of the immovability of the cable saddles, any unbalanced horizontal cable pull must be resisted by the towers in bending. The calculations indicate resulting compression stresses in the masonry, at the floor level, of more than 700 lb per sq in.—about 50% of which is due to bending. In view of the excellent condition of the masonry, these stresses may be considered per-

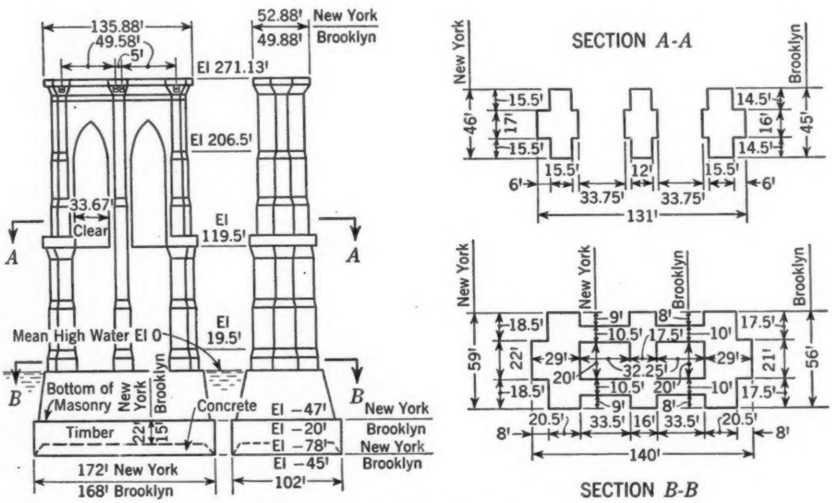


FIG. 17.—THE TOWERS

missible, but in any major reconstruction they should not be allowed to increase. Such an increase can be prevented by reconstructing the cable saddles to assure longitudinal motion and thus avoid bending the towers.

The pressure at the bottom of the New York tower foundation, under balanced cable pull, is calculated at 5.5 tons per sq ft; and the maximum edge pressure under unbalanced cable pull, at 9 tons. The pressures under the Brooklyn tower are somewhat less. There is no evidence that any settlements have occurred; but, like the stresses in the masonry, the foundation pressures should not be increased.

The Anchorages.—The anchorages form massive limestone masonry blocks about 90 ft high (Fig. 18). They rest on shallow timber grillages about 130 ft long and 120 ft wide, originally placed entirely below the ground-water level. The anchor chains, from their attachment to the cables at the top of the anchorage, pass through the anchor block with a right-angle arc. At the bottom they are attached to cast-iron anchor plates. The anchor chains are composed of forged wrought-iron bars and, except for a short piece at the upper end, are completely embedded in the masonry. The cable stress be-

comes gradually dissipated by friction in the masonry and the partly exposed upper links of the chain may be considered the most critical ones.

The anchorage bars had been made for a specified strength of 50,000 lb per sq in., and a test coupon cut from a similar forged erection bar which had been left in place showed a strength of 48,000 lb per sq in., whereas the maximum stress in the anchor bars under a live load of 2,400 lb per ft is only 12,000 lb per sq in. A piece exposed by removing the surrounding concrete revealed the bar in an excellent state of preservation. The anchor chains, like the cables proper, would therefore be able to resist, safely, a considerable increase in load.

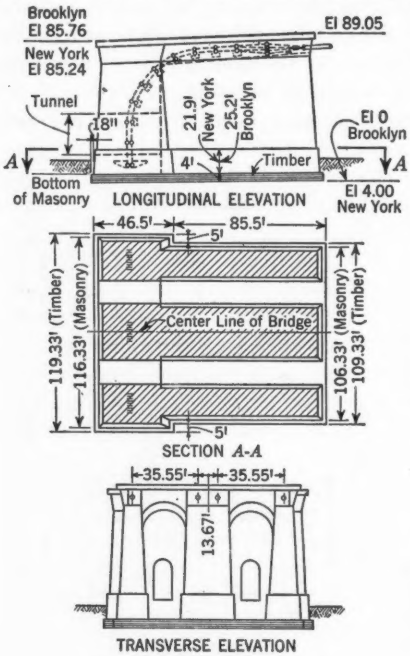


FIG. 18.—THE ANCHORAGES

The maximum toe pressure of the anchor block on the underlying compact sand formation is estimated at about 6.5 tons per sq ft. This appears somewhat high. In fact, slight settlements are indicated, but there is evidence that they occurred mostly during or soon after construction. Nevertheless, it was thought that the settlement might be due partly to a deterioration of the timber platform on which the anchor blocks rest, and in 1943 it was decided to excavate pits to the bottom of the base to permit examination of the timber. Although the ground water was lowered, so that on the New York side the timber platform is partly, and on the Brooklyn side entirely, above the present water level, the condition of the timber was perfectly sound. The masonry likewise is in excellent condition so that there is sufficient cause for confidence that even a moderate increase in pressure would not result in further settlements or in any way endanger the stability of the anchorages.

POSSIBILITIES FOR RECONSTRUCTION TO SERVE SIX-LANE HIGHWAY TRAFFIC

A simple plan of alteration has been developed by which the bridge can accommodate two 3-lane roadways for passenger automobiles and buses when the surface car tracks are removed (Fig. 19). The scheme involves the removal of the top chord and web members of the two intermediate trusses and their re-erection in place of the two shallow outer trusses. The top chords of each remaining pair of trusses are to be connected by knee-braced struts. The trolley tracks and the present wooden roadway flooring would be replaced by a light open-grid steel floor over the full width of each 3-lane roadway.

The cost involved in this alteration is very moderate and the changed condition is expected to be adequate for many years, especially since the present approaches and street connections make continued exclusion of truck

traffic advisable. It is estimated that the present capacity volume of vehicular traffic would be more than doubled.

A tentative study also indicates that it will be feasible to reconstruct the bridge for 6-lane unrestricted highway traffic, corresponding to a live load of 3,000 lb per ft of bridge. By the use of a light floor, the increased load would be resisted safely by the cables, towers, and anchorages, provided the cable saddles on top of the towers are rebuilt to assure longitudinal motion under live load and temperature changes. It is questionable whether this can be done at the same time making the necessary floor alterations economically

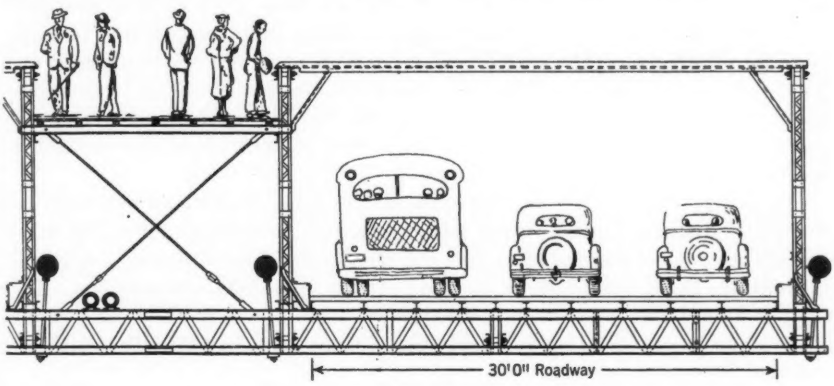


FIG. 19

while the cables remain in place. It may be found more economical to rebuild the cables completely. Whether the diagonal stays would be retained is largely a question of possible sentiment in favor of preserving the characteristic appearance of the bridge.

On account of excessive stresses in the stiffening trusses and floor system, complete reconstruction of the suspended structure would be necessary.

CONCLUSION

Despite the crudeness of the theories employed in its design, the incongruities of some of the design features, primitive structural details, and consequent defects which developed during the life of the Brooklyn Bridge, this structure has given greater service than was originally anticipated.

Its behavior is a testimony to the fundamental soundness and safety of its design and that of the suspension type of bridge in general. The bridge as it exists today cannot be considered serviceable for unrestricted modern highway traffic, with two tracks for electric surface cars; but with moderate structural changes it can be reconditioned to serve a greatly increased volume of automobile traffic for many years.

A complete reconstruction of the suspended structure, with retention of the towers and anchorages, and possibly even the cables, to transform it into a modern 6-lane highway bridge for unrestricted traffic is feasible and can be accomplished without altering the general appearance of this magnificent structure, which has become world renowned and a cherished landmark of the City of New York.

PHYSICAL EXAMINATION AND FIELD OBSERVATIONS

By Nomer Gray,[12] Assoc. M. ASCE

Introduction

The need for a physical examination in an investigation of this kind is self-evident. Parts of the Brooklyn Bridge are more than seventy years old. For a great part of the life of the bridge, the loads were considerably in excess of those originally contemplated. The location is an unusually exposed one. At the time of construction, steel was just beginning to be used in large bridges and there was no general agreement on the best specifications governing its manufacture. In the light of these facts, the present physical condition of the bridge is of general interest.

In a structure as old as Brooklyn Bridge, the original adjustments are frequently disturbed as a result of settlement, added dead load, or maintenance operations. Such changes can affect the stress condition materially, and stress calculations become dependent, to some extent, upon field observations. It is the purpose of this paper to present, briefly, some of the methods and results of the physical examination and field observations made in connection with this study of Brooklyn Bridge.

The datum originally used in the construction of the bridge was mean high water at the site, as established by tide gage observations made by the bridge engineers. It was considered advisable to refer the 1943 surveys to this original datum. Unfortunately, no records of old bench marks were found, and many discrepancies arose in attempting to reconcile old plan elevations with the 1943 level runs. This problem was solved satisfactorily by reference to a very comprehensive set of precise levels[13] made about 1910 by the City of New York for the purpose of establishing one datum for all city departments. Library records of this excellent work provided ties with the datum planes used by the borough governments. The recorded relationship between the datum planes of Brooklyn Bridge and those of the Borough of Brooklyn and of the Borough of Manhattan controlled the level surveys. The first levels taken were for anchorage settlement.

Settlement Observations

The two anchorages are of limestone masonry, laid with joints of Rosendale cement mortar. Their construction is identical, except that the base of the masonry of the Manhattan anchorage is 4 ft lower than the base of the masonry of the Brooklyn anchorage. Both are supported on timber grillages 4 ft thick which, in turn, rest upon well-compacted sand. Close examination of the masonry does not reveal any cracks or spalls, and the original mortar of the

[12] Civ. Engr., Dept. of Public Works, City of New York, New York, N. Y.

[13] "Precise Levels in New York City," by F. W. Koop, Board of Estimate and Apportionment, New York, N. Y., 1915.

exterior joints is in excellent condition after 70 years. A level check of a bed joint near the ground surface at the Brooklyn anchorage showed an extreme variation in elevation of 3 in. and an average settlement, from plan elevation, of 5 in.

In the absence of exact information regarding allowances made, during construction, for anticipated initial settlement, a comparison between lower-course elevations and plan elevations can be misleading. A level check with the top course is more dependable, since corrections in the estimated settlement during construction will have been made to bring the upper courses closer to plan grade. The average settlement of the Brooklyn anchorage based upon top-course elevations was 3 in. From this evidence it appears that the anchorage settled about 2 in. during construction. The observed settlement and tilt of the Brooklyn anchorage, coupled with the knowledge that the bridge rested on a timber grillage, aroused suspicion of the condition of the timber. Old construction records showed the ground-water level to be just below the top of the grillage. It is well known that the ground-water level has been lowered materially in Brooklyn. This was confirmed by a test boring which showed a drop of 13 ft, placing the present ground-water level 11 ft below the grillage and, incidentally, 11 ft below mean high water. Thus, it became necessary to examine the grillage.

A test pit 26 ft deep was excavated at the face of this anchorage, at the point of greatest settlement. When the concrete encasement was stripped off, the yellow pine 12-in. by 12-in. timbers were found to be in an excellent state of preservation, except for the first inch penetrated. It had been intended to take horizontal cores from the timber grillage, using a standard test boring rig placed in the bottom of the test pit, but great difficulty was encountered because of numerous drift bolts in the timber. Hand-operated auger borings, 5 ft deep, were made, without revealing any change in the quality of the chips. The auger holes may be seen in the exposed timbers in Fig. 20.

Conditions at the New York anchorage were better. The settlement (5 in.) was uniform judging by the lower-course elevations, and only 1.5 in. as measured by the top-course elevations. The present ground-water level is about 8 in. below the top of the grillage, which (as nearly as could be observed) is kept completely wet by capillary action. Auger borings showed sound wood in the accessible upper courses of the grillage. There is additional evidence that most of the settlement in both anchorages occurred during construction or shortly thereafter.

Physical Condition of Structural Members

The exposed ends of the wrought-iron eyebars in the anchorage galleries showed no sign of corrosion. At the point where the eyebars enter the wall, concrete was removed for a depth of several inches. The original coating of red-lead paint on the bars was found to be intact. The massiveness of the masonry towers (Fig. 7) contrasts strongly with the relative slenderness and flexibility of modern steel towers. Some conception of the size of the towers may be gained from the fact that the respective weights are 93,000 tons and 108,000 tons, each being considerably heavier than the 60,000-ton anchorages.

The combined weight is about twelve times that of the suspended superstructure.

The tower masonry is almost entirely of granite. Although subjected in places to appreciable stresses, there is no apparent sign of distress in the masonry. With the exception of one or two places where the stone has been exposed to an unusual amount of wetting and drying, the mortar of the original joints is in excellent condition.

As in the case of the anchorages, the two towers rest upon heavy timber grillages 15 ft and 22 ft thick, respectively. Their great thickness is explained

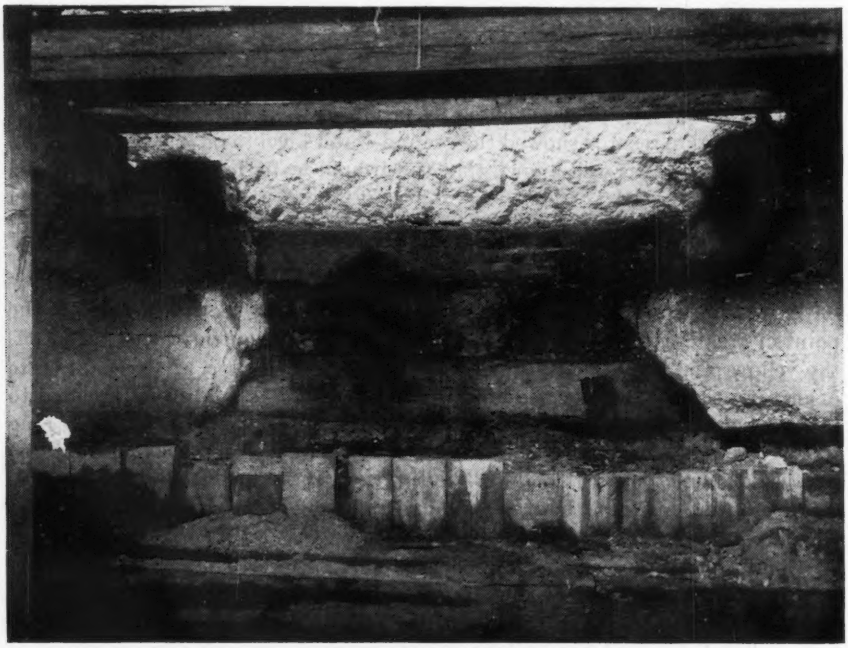


Fig. 20.—Edge of a Timber Crib Under the Brooklyn Anchorage

by the fact that they originally formed the tops of the compressed air caissons used in construction, and as such were designed as slabs to bear tremendous loads. Immediately below the grillage there is a mass of concrete, from 6 ft to 7 ft thick, filling what was formerly the working chamber of the caisson.

At the site of the Brooklyn tower, rock lies more than 90 ft deep. The difficulty of excavating and the fact that the material encountered gave evidence of a bearing capacity greater than 20 tons per sq ft led Washington A. Roebling to found the Brooklyn tower on compact sand, gravel, and boulders at El. − 45.

The Manhattan tower is generally regarded as resting on rock. Old reports indicate that the rock surface under the tower varies from El. − 75 to El. − 90. The caisson cutting edge is at El. − 78 and the concrete fill bears upon rock

projections and hardpan, filling the depressions in the rock. The direct bearing pressure of about 7 tons is conservative for these materials.

The situation at the Manhattan anchorage with respect to the tower grillages differs materially from that at the Brooklyn anchorage. The tops of the grillages are, respectively, 20.0 ft and 46.5 ft below mean high water. Soundings indicate that the river bottom adjacent to the towers is 20 ft and 34 ft above the respective grillages. At these depths, the timber, which is completely encased in concrete, should be entirely safe from attack by marine organisms.

Surveys to determine plumbness and settlement of the towers confirm this judgment of the state of the grillages. No settlement or tilting was detected in either tower. The towers, which are higher than 300 ft (Fig. 17), are plumb within 0.5 in.

Movement of Cable Saddles

The cable saddles at the tower tops each rest upon about forty cylindrical rollers, 3.5 in. in diameter. This design had been adopted principally to provide for saddle movement during erection. The base plates were placed 12 in. shoreward and the saddles were originally an additional 6 in. shoreward from the transverse center line of tower. The positions of the saddles at various times are given in Table 1.

TABLE 1.—Riverward Movement of Cable Saddles A, B, C, and D, Fig. 8, from Their Original Positions[a]

Date	Brooklyn Tower				Manhattan Tower				Averages[b]	
	A	B	C	D	A	B	C	D	Total	Operation
May 21, 1883[c]	3-11	3-9	3-10	3-6	3-5	3-12	3-12	3-4	3-9	0
May 26, 1883	4-9	4-10	4-11	4-10	3-8	3-15	3-15	3-5	4-2	0-9
May 2, 1898[d]	5-4	5-6	5-2	4-14	3-12	5-0	5-2	4-10	4-14	1-5
July 30, 1898[e]	6-8	7-10	7-8	8-2	3-12	5-14	6-6	6-12	6-9	3-0
June, 1902	6-0	6-10	6-8	7-1	4-2	5-6	5-7	5-1	5-12	2-3
June, 1915	6-0	6-13	6-10	7-2	4-8	5-6	5-6	5-2	5-14	2-5
June, 1943	7-12	7-8	8-8	8-0	5-14	6-12	6-12	6-10	7-3	3-10

[a] The original position of each saddle was 18 in. shoreward from the transverse center line of the tower. Dimensions are in inches and sixteenths—for example, "3-11" denotes $3\frac{11}{16}$ in. [b] The column of totals gives the average total movements from the beginning of construction to the date indicated. The column marked "operation" gives the average total movements during the time the bridge has been in operation. [c] Two days before the bridge was opened. [d] The first year that trolley cars were operated on the roadways. [e] Immediately after the bottom chords buckled.

Col. Washington A. Roebling stated[14] that the sensitiveness of the saddles under unequal loading, during the erection of the superstructure was quite marked but that the saddle movement in the completed structure, under ordinary traffic, would be very small indeed. This proved to be the case, although under increasing dead loads and live loads the saddles moved very gradually toward the river.

[14] "Report of the Chief Engineer on the Strength of the Cables and Suspended Superstructure of the Bridge to the Board of Trustees, January 9, 1882," Eagle Book and Job Printing Dept., Brooklyn, N. Y., 1882.

During erection of the cables and superstructure, the saddles moved an average of $3\frac{9}{16}$ in. toward the river. After the bridge was opened their movement, to June, 1943, has amounted to an additional $3\frac{5}{8}$ in. Until 1887 movements of the saddles under seasonal temperature changes were observable and amounted to from $\frac{3}{8}$ in. to 0, indicating a variation in rolling friction for the various saddles.

Since 1902 the saddle movement has been small and very gradual except in 1922 when two saddles moved suddenly under heavy vehicular traffic and high temperature. Although records of the exact movements since 1915 are lacking, an examination of the saddles indicates that they have not moved for years. The likelihood of movement is dependent, of course, upon the magnitude of unbalanced side and main span cable pulls. The friction is now very great as a result of a skewing of some of the rollers and an accumulation of rust and dirt, the rollers being practically inaccessible. No movement occurred under a fairly heavy unbalanced test load.

Live loads and temperature changes tend to produce unbalanced cable pulls which, in modern flexible steel towers, are compensated for by movements of the tower tops. This is not so, however, in the masonry towers of the Brooklyn Bridge. As a result of saddle friction and failure of the massive towers to yield easily, the unbalanced cable pulls induce appreciable bending stresses in the towers. The determination of the magnitude of these stresses depends to some extent upon the movement of the tower tops. Analytically, this is a complex problem. It was a relatively simple matter, however, to measure the movement by observations on a full-scale model which, in effect, is what the investigators had in this case. A transit was set at the base of the tower on the prolongation of the transverse center line. Empty elevated trains weighing a total of 600 tons, centrally placed on one span at a time, were used. Movements of the Brooklyn tower top of approximately $1\frac{1}{4}$ in. and $1\frac{1}{2}$ in., respectively, were observed for the load on the main and side spans. Further transit observations revealed that the elastic line of the tower, with the top so deflected, was practically a straight line—a significant phenomenon, as it indicates that the tower rotates principally as a rigid body, about the base. This angular yielding of the base reduces the bending stresses in the masonry.

The towers, generally, were found to be in excellent condition, testifying to the soundness of their construction and the durability of good stone masonry.

The Main Cables

The main cables are the principal supporting members and, as such, merit careful examination. These cables, four in number, are $15\frac{5}{8}$ in. in diameter under the wire wrapping. The exact net area of the cables is not known; but, taking the best estimate as 144.8 sq in., the percentage of voids is 24%. This may be compared with from 19% to 21% of voids in modern cable construction.

To examine the cables in detail, it was necessary to remove the wire wrapping at a number of places. On this bridge, the cable bands were placed over the wrapping, which is continuous from anchorage to anchorage, except through the towers. The wrapping, which originally had been placed by hand, was found to be tight and generally well painted. The watertightness of the

painted wrapping is indicated by the fact that the interior of the cables is dry at the low points. In the worst case after several days of rain, a slight moisture was detected in one cable at the midpoint of the main span.

The wrapping wire was removed from the cables for lengths of from 7 ft to 10 ft at twenty-four points; twelve cable wires were removed at each point—or 0.2% of the cross section of the cable. In each case, a length of new wire was spliced in to replace the wire removed for test. It was possible to remove and

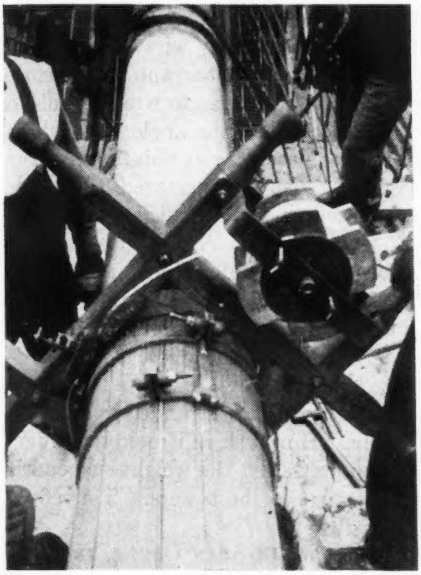


FIG. 21.—DEVICE FOR REWRAPPING THE CABLE BY HAND

FIG. 22.—LONG OVERFLOOR STAY ROPES FROM THE CENTER LINE OF THE BRIDGE

splice in surface wires within 1 ft of the end of the wrapping. An effort was made to achieve approximately the original tension in each spliced wire by screwing up the threaded sleeve splice. The method of obtaining the correct tension (admittedly not exact) was to approximate the force required to deflect the wire from its straight alinement.

Under the wrapping wire, a loose, dry, chalky substance (which was probably the white lead of the paint applied to the wrapping) appeared between the surface cable wires. When this was brushed away, the cable wires were found to be almost entirely free from rust. At a few points, notably near the center of the main span, some of the surface wires were rusted just under the cable band. This condition rarely extended to a depth, into the cable, of more than two wires. The writer attributes the rusting at these points to two factors: (1) The very short suspender rods, at these points of maximum truss expansion, cause some "working" of the cable bands which tends to abrade the surface wires; and (2) canvas fillers under the cable bands at these points undoubtedly contribute to the retention of moisture and consequent promotion of rust.

For some distance out from the anchorages the deck is supported on top of the cables by short posts. Because of the greater rigidity of this connection between cable and deck, the "working" of the cable bands has caused the wrapping wires to wear ridges into the top surface wires of the main cables and in some cases to break a few of them. The cables have been inspected regularly at these points and the broken wires repaired.

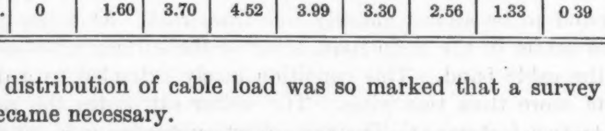
The cables were wedged open to a depth of about 4 in. to permit visual examination of the interior wires at many points. In every instance the interior wires appeared as clean as, and generally cleaner than, the surface wires.

The hand-wrapping frame for replacing the wrapping wire is shown in Fig. 21. Its operation will be self-evident from the photograph. For close contact between adjacent wires the practice, of course, was to wrap uphill—a fact which is not apparent in the photograph because of the angle from which it was taken. The clamps around the cable were necessary at points where the cable had been wedged open, as the wedging usually increased the cable diameter slightly. The wrapping frame was used wherever sufficient clearance permitted its use.

The physical characteristics of the cable wire, as determined from many tests, are the subject of the fourth and final paper in the Symposium.

The shape of the cable curve was a matter of interest, differing as it does from the conventional form. In most modern suspension bridges, the approximately uniformly distributed suspended weight results in an equilibrium polygon very close to a true parabola. Actually, in the stress computations for a truss it is commonly treated as a parabola. However, in Brooklyn Bridge, the effect of the overfloor stays is to reduce, appreciably, the weight suspended from the main cables in the quarter span adjacent to the towers. This effect

TABLE 2.—Ordinate Measurements to the Main Span Cable, in Feet



Description	Panel Points									
	T	6	18	30	42	54	66	78	90	104
Brooklyn side......	131.33	113.01	89.65	68.25	48.79	32.41	19.43	9.25	2.71	0
Manhattan side....	131.39	113.05	89.39	67.89	48.42	32.25	19.23	9.13	2.62	0
Variation from parabola............	0	1.60	3.70	4.52	3.99	3.30	2.56	1.33	0 39	0

of nonuniform distribution of cable load was so marked that a survey of the cable curves became necessary.

Two factors tended to complicate the cable-curve survey: (*a*) The curve changes from minute to minute under passing train loads; and (*b*) there is the usual uncertainty regarding the true cable temperature during the daylight

hours. By making the survey at night, both these difficulties could be overcome, because the train service could be discontinued. However, the difficulty of measuring cable ordinates accurately, in total darkness, will be readily appreciated. The solution of the problem was to make a profile survey of the more accessible top chord of the truss between midnight and dawn, with the train service stopped. With the profile of the top chord as a reference line, it was an easy matter to measure the vertical distances from the cable center line to the top chord at any convenient time, even with trains running. The resulting cable curve, of course, would be that which existed at the time the chord profile survey was made. It may be of interest to record that passing trains produced a deflection at the center of the main span in excess of 1 ft. Their observed effect in elongating suspenders did not exceed $\frac{3}{16}$ in.

Referring to Table 2, the effect of the stays in distorting the curve from parabolic form is clearly discernible. The parabola used for comparison is that which would contain the points of intersection of the tangents at the towers and the low point of the midspan. The measured ordinates were used in the truss stress analysis.

Stiffening Effect of Stay Ropes

One of the distinguishing features of Brooklyn Bridge is the use of overfloor stays to stiffen the suspended structure. These are wire ropes, from 1.75 in. to 2 in. in diameter, which radiate from the tower tops to connect with the trusses at two-panel intervals in the quarter span adjacent to the towers. A view of the stays looking along the center line of bridge is given in Fig. 22. The stays are clipped to the suspenders at every intersection. The flatness of the catenaries of the long stays emphasizes more clearly than figures the high tensions of the stays.

The present condition of the ropes is fair considering their long service in an extremely exposed location and the constantly varying tensions under passing loads. Care has been taken to prevent loss of section by the use of metal sleeves or leather spacers at points of contact with other surfaces. A tensile test was made on a single $1\frac{7}{8}$-in. stay rope, removed from the bridge for this purpose. This stay rope, which had been in service for 60 years, developed an ultimate strength of 147,000 lb per sq in. The modulus of elasticity was approximately 20,000,000 lb per sq in.

The tensions in the stays were set at from 15 tons to 17 tons each in the original design. Knowledge of the present stay tensions was necessary to determine the thrust induced in the bottom chords of the trusses to which they are attached. The cumulative effect of these stay tensions is surprisingly large, being of the order of 500,000 lb in a single bottom chord.

Observations were made to determine the tension in thirty two of the one hundred stays which lie under one cable. As will be appreciated by any one who has ever used a surveyor's tape, the tension in a rope is a function of the sag. The stay tensions were found by measuring the vertical sags. The method, as indicated in Fig. 23, is as follows:

A transit is set up adjacent to the lower end of the stay, the horizontal axis being on a level with a point on the stay. The telescope is directed along the

closing chord, by sighting at the upper end of the stay; and then it is clamped, fixing the vertical angle, which is recorded. Next, the transit is placed, by trial, in a new position so that the line of sight is tangent to the stay, while maintaining the original vertical angle. The method of calculating the desired vertical sag will be evident from Fig. 23. In this manner, it is possible to make all the observations without leaving the bridge floor. The entire operation was frequently completed in 20 min, using one observer and one assistant.

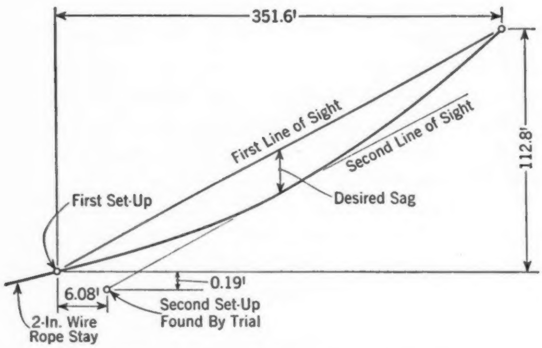


FIG. 23.—METHOD OF MEASURING STAY-ROPE STRESSES (Measured Sag 2.14 Ft and Computed Stress 50.8 Kips Per Sq In.)

The observations were necessarily made at temperatures which differed considerably from the basic temperature of 39° F used in the stress calculations. Therefore, corrections had to be applied to the stay tensions as calculated from the observed sags. When temperatures change, several factors influence the stay tension, as follows:

(1) The stay rope itself tends to change length;
(2) The curve of the main cable changes as the cable length changes; and
(3) The truss shortens or lengthens, thus moving the stay connection horizontally and influencing the stay tension.

The effect of factor (2) is to raise or lower the truss to which the lower end of the stay is connected, thus influencing the tension. Factors (1) and (3) can be treated analytically, but factor (2) offers great difficulty analytically because of the interaction of a number of elements. It proved fairly simple to observe the elevation of the truss at the two temperatures—namely, at 39° F and the temperature prevailing when the sag observation was made. For the long stays, the difference in elevation amounted to as much as 5 in. for a 50° F change in temperature. The increment in stay tension was as great as 23% for a 50° F rise in temperature.

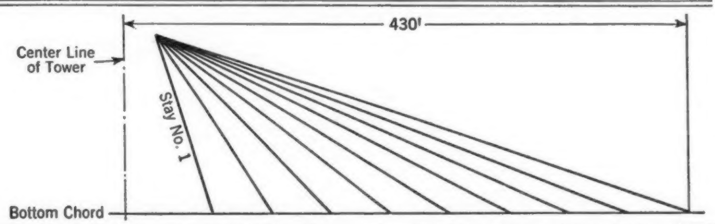
A comparison of the design tensions and the measured tensions is given in Table 3. In maintenance operations on the stays, through the years, the practice has been to turn the adjusting nuts back to their original positions, which procedure, if faithfully followed, should have preserved the initial tensions. The slightly higher present tensions are probably caused by increases in the dead load since the time of the original adjustment.

STIFFENING TRUSSES

The stiffening trusses have tension web members, with adjustable counter diagonals pin-connected to the chords. The small size of the members, the

nature of the details, and the absence of traveling platforms combine to make painting costly. However, inspection indicates that, through most of its long life, the bridge has been well painted. There is no marked reduction in sectional area through corrosion, except at the towers. At this point, curiously enough, the chords lie partly in chases, in the granite, making access for painting impossible. Fortunately, the sectional area is greatest here and the same loss of area is not as significant as it would be elsewhere. Nevertheless, it will be necessary to strengthen the corroded sections of the bottom chords at the towers.

TABLE 3.—TENSIONS MEASURED IN THE STAY ROPES, IN KIPS



Item	Description	Stay No.:							
		4	7	10	13	16	19	22	25
1	Panel point	13	19	25	31	37	43	49	55
2	Diameter, in inches	1.75	1.75	1.875	1.875	1.875	1.875	2	2
3	Original tension, probably at 90° F.	33.3	33.3	30	30	30	30	30	30
	Present Dead-Load Tension, at 39° F:								
4	Brooklyn end; side span	21.7	27.4	28.4	21.8	23.2	30.1	32.9	31.2
5	Brooklyn end; main span	26.3	22.7	25.3	24.8	24.9	32.9	37.8	41.4
6	Manhattan end; side span	19.4	20.5	29.6	27.1	27.0	29.7	37.1	33.3
7	Manhattan end; main span	30.2	30.8	22.6	22.4	19.8	28.4	33.9	28.8

In years past, the deflections under live load, common to suspension bridges, have resulted in considerable pin wear. During those years, the pins have been inspected and replaced where necessary as a regular maintenance practice and, in consequence, their present condition is not bad.

During the first decade of the twentieth century, the bridge was subjected to very much heavier live loads than at present. At that time the diagonals frequently failed and the top flanges of the floor beams cracked. Since 1922 no heavy trucks have been permitted on the roadways and the elevated train service has decreased progressively until, in March, 1944, train service was discontinued. Under the prevailing light live loads, the aforementioned difficulties have practically disappeared.

Analysis showed that the chord stresses were high, and it was decided to test the physical characteristics of the chord materials. Since it was impracticable to remove a full-chord section, coupons were cut from the flanges of the channels making up the chord section at sixteen well-distributed points. They were cut out with hack saw and chisel, sufficient excess being taken to

permit machining away the material disturbed in the cutting. The results of these tests are reported in the last paper of this Symposium.

As stated, the cumulative tensions of the overfloor stays, applied to the bottom chords of the stiffening trusses, result in a large compressive force near the towers. At a point about 250 ft from the towers, the bottom chord has a low sectional area (13.72 sq in.). The compressive unit stress from the stays alone reaches a high value at this point. It was realized that other longitudinal members must participate in resisting this compressive force. The best approach to the determination of the extent of the relief afforded by other members appeared to be a direct field measurement of the bottom-chord stress by strain gage. To obtain a zero stress gage reading, auxiliary channels were riveted and welded to the top and bottom of the existing bottom chord and zero stress readings were taken on the new channels after they were in place. This arrangement with the new channels in place is shown in Fig. 24. Six gage

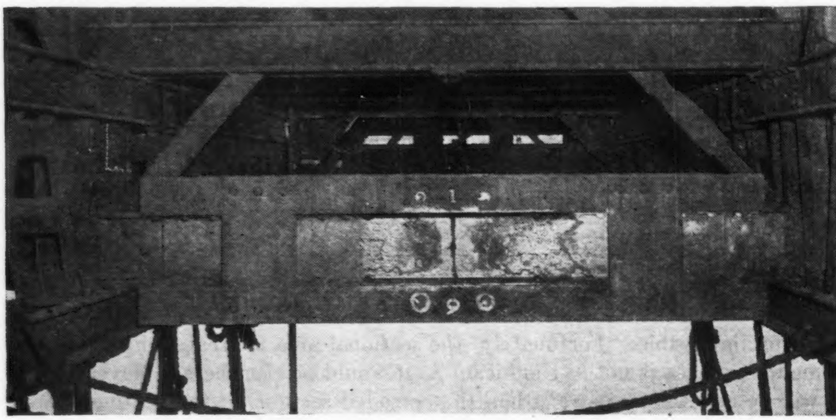


FIG. 24.—BOTTOM CHORD OF STIFFENING TRUSS WITH AUXILIARY CHANNELS IN PLACE

lines were used on each new channel, completely traversing the section in order to obtain a fair total stress.

The original chord section was then burned through at the middle of the panel, transferring the compressive stress into the new channels. In manipulating the torch care was taken to remove metal concentrically and progressively, in order to avoid high temporary eccentric stresses. The strain-gage measurements then taken on the new channels gave a unit stress of 17,000 lb per sq in. for the original chord section. This value is about 30% less than it should have been for the then existing loading condition, which indicates that other members participate in the bottom-chord stress to that extent. In this manner, it was possible to obtain a more exact knowledge of the actual bottom-chord stresses, confirming an analytical treatment of participation.

On July 30, 1898 (see Table 1), a traffic block on one roadway caused a large overload and a partial failure of the bottom chords of the stiffening trusses, resulting in considerable discussion in the technical journals of that

period. The bottom chords of four main-span trusses were bent out of line, because of the extremely high compressive stress caused by the stays. In time, the same bending was produced in the side spans. The bending referred to is

FIG. 25.—VIEW ALONG THE TRACK, SHOWING THE BEND IN THE FLOOR STRUCTURE AFTER CHORD MEMBERS HAD BUCKLED

clearly visible in the single plank walk shown in Fig. 25. Although the misalinement was never corrected, the chords were reinforced with cover plates and bottom battens about 1908.

TABLE 4.—TENSILE TESTS ON EYEBAR DIAGONALS

Item	Description	Bar 1	Bar 2	Bar 3	Bar 4
	Nominal Section, in Inches:				
1	Width	3	2	2	2
2	Thickness	0.625	0.5	0.5	0.5
3	Measured areas, in square inches	1.93	1.12	1.12	1.14
4	Yield point, in kips per square inch	44.6	42.7	44.9	46.1
5	Ultimate strength, in kips per square inch	75.8	69.2	71.8	71.3
6	Ultimate percentage elongation in 22.5 ft	13.7	9.28	13.5	14.0
7	Percentage reduction of area	46	41	46	53

In order to learn something of the physical characteristics of the web members, four eyebar diagonals were removed from the high trusses for full-size tensile tests. The results of these tests are presented in Table 4. None of the eyebars broke in the head, a considerable excess sectional area having been provided in their manufacture. This excess is made up partly by additional

thickness in the head, since there is insufficient clearance within the chords for widening the head. The additional thickness is fortunate, since corrosion is greatest in the eyebar head, which is inaccessible for painting.

Decision to remove the eyebars for test provided an opportunity to check their stresses by strain gage. Accordingly, gage points were drilled in the members to be removed, and readings were taken at a moment of extremely light live load. Theoretically, there should be only nominal dead-load stresses in a properly adjusted truss at mean temperature, and the analysis indicated small live-load stresses. Surprisingly, on measuring the no-load gage lengths after removal from the truss, the stresses in the diagonals were found to be approximately 24,000 lb per sq in.

The four eyebars were removed from points well spaced along the main span. The discovery of this high shear could not be explained satisfactorily by the general truss analysis. However, it was in agreement with the records of failures of many diagonals in past years. After some study, it became apparent that these stresses could be caused by high tensions in the adjustable counterdiagonals.

To test this hypothesis, gage holes were drilled into the main diagonals and counters of three adjacent panels at two widely separated points. The gage distances in the counters were measured before and after slacking off the

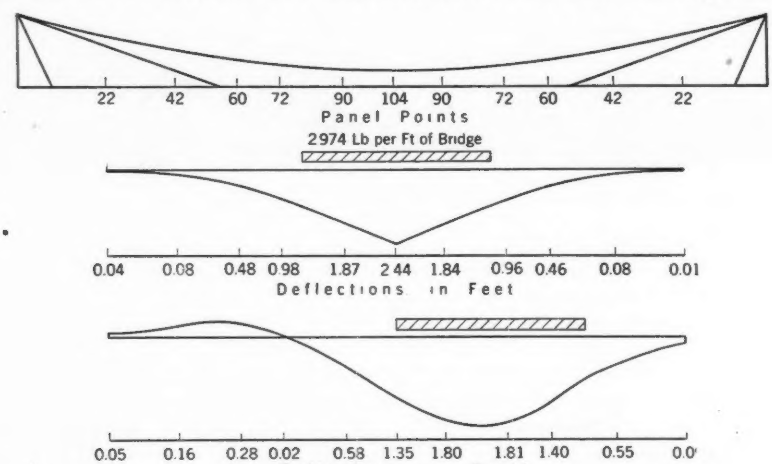


FIG. 26.—DEFLECTIONS OF THE MAIN SPAN UNDER LIVE LOAD
(One Eight-Car Train on Each Track; Total Length, 394 Ft and Total Load, 586 Tons)

adjusting sleeves to zero tension. As had been supposed, the countertensions were found to be unnecessarily high—from 12,000 lb per sq in. to 17,000 lb per sq in. Simultaneous readings on the adjacent main diagonals showed smaller changes in their tensions—suggesting that the horizontal shear produced by the over-tightened adjustable counters is resisted over long lengths by groups of diagonals rather than by the immediately adjacent main diagonal. Thus, efforts to correct this condition will require the scheduling of the adjustments and a large amount of checking and rechecking by the strain gage.

Live-Load Deflections

In the course of the analysis of truss stresses, by an adaptation of the so-called elastic theory, the need arose for a check on the deflections under live loads. Arrangements were made to place trains in predetermined positions on the main span and side spans. Profile surveys of the top chord were made before and while the trains were in position. The resulting deflections are shown in Fig. 26 and Fig. 27.

Historically, the introduction of the Melan deflection theory into the United States is associated with Brooklyn Bridge. The late Mr. Moisseiff (who is

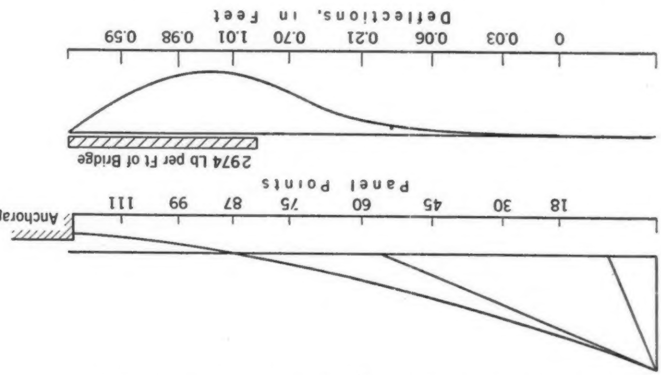


Fig. 27.—Deflections of a Side Span Under Live Load
(One Six-Car Train on Each Track; Total Length, 295 Ft and Total Load, 439 Tons)

generally credited with the first application of this theory to an actual structure) has stated that it was his study of the failure of the suspender rods of this bridge in 1901, which first drew his attention to the effect of the distributed dead load in resisting live-load deflections.

Further checks on the live-load chord stresses were made with the strain gage, using the train loads. The results are too detailed for enumeration in this paper. It may suffice to state that the strain gage showed stresses lower than those computed by the elastic theory and the measurements aided in the determination of stress reduction percentages.

The Floor System

The floor beams are continuous over the four cable supports. The solution of the floor-beam stresses is complicated by consideration of the amount these four supports yield under load. The exact calculation of the deflections of the six trusses is an extremely difficult, if not impossible, problem if approached from a purely analytical angle because of the interaction of the main cables, six trusses, the overfloor stays, and the floor beams. A more direct approach is to select that floor beam which will deflect the most, because of differential deflections of the supporting cables, and measure the deflection under a known load. This was done, using the same train loads as shown in Fig. 26. At midspan the inner cables and trusses deflected 2.5 in. more than the outer trusses, under

the centrally placed load. After the yielding of the floor-beam supports under a known load had been measured, it was possible to calculate the floor-beam stresses for the working loads used in the study.

Conclusion

The Brooklyn Bridge has had a long and extremely useful life; and, although it is customary to associate old age with decrepitude, there is nothing in the results of the physical examination to suggest that advancing age has seriously weakened any major element in the structure. Stone masonry plays a larger part in this bridge than in modern long-span suspension bridges, and the durability of such construction, when properly executed, is well known. The lasting quality of steel in a well-designed bridge is almost entirely a matter of protection from the elements. The examination has shown the bridge to have been well painted during the major part of its life.

The bridge is statically indeterminate to a high degree. The analytical approach to its stress condition is very complex. It was logical that direct measurements should be taken whenever possible, as an aid to stress determination. Seldom does a bridge designer have a full-scale model before him on which to test the accuracy of his simplifying assumptions. Much could still be learned from a judicious application of the strain gage.

In closing, the writer feels impelled to call attention to the great value of detailed construction reports on large projects. To know all about a structure, an engineer must look further than the final plans and specifications. The progress reports of the chief engineer and his several assistants on the construction of Brooklyn Bridge are veritable mines of information which are still available in technical libraries. In the course of this investigation, they helped resolve many questions; and they can be read today, with profit, by the practicing engineer as well as by the student of bridge engineering.

TESTS OF METALS REMOVED FROM CABLES AND STIFFENING TRUSSES

By Harold E. Wessman,[15] M. ASCE

Introduction

The tests reported in this paper were made on wire specimens cut from the cables and on chord specimens cut from the stiffening trusses of the Brooklyn Bridge. As stated in the other papers of this Symposium, this historic structure was opened to traffic in 1883; and, consequently, the test specimens have been in service for at least 60 years.

Cable Wire

Wire samples were cut from the top, side, and bottom of each of the four cables at six different points on each cable as indicated in Fig. 28. Of the total, sixty samples were subjected to tensile tests to failure. The ultimate strength, elongation, and reduction in area were determined for all the specimens. In addition, strain measurements over an 8-in. gage length were taken on twenty four of the sixty samples and stress-strain diagrams plotted to determine the proportional limit, yield strength, and modulus of elasticity. The wire samples when cut from the main cables tended to assume immediately the curvature of the coils in which they were delivered to the bridge site years ago. Hence, after being placed in the grips of the testing machine, the wire was subjected to an initial load of 250 lb to straighten it before the extensometer was attached.

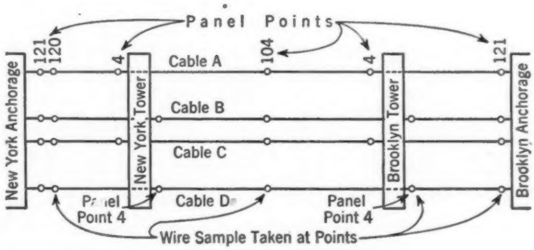


Fig. 28

Simultaneous load and strain measurements were also taken on twelve long samples using a gage length of 84 in. to obtain a more accurate value for the modulus of elasticity. In these tests, the maximum load was approximately that corresponding to the proportional limit of the material.

Values for the physical properties of the cable wire are summarized in Table 5, Cols. 2 to 5. The values in Cols. 4, 8, and 10, Table 5, are arithmetical averages. Maximum and minimum values are also tabulated in order to indicate the range of values encountered among the samples tested.

The average "ultimate tensile strength" for sixty samples is 162,600 lb per sq in. The original specifications,[16] phrased in terms of the small-diameter

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[16] "Specifications for Steel Cable Wire for East River Suspension Bridge," in "Report of the Chief Engineer of the New York and Brooklyn Bridge, January 1, 1877," Eagle Book and Job Printing Dept., Brooklyn, N. Y., 1877, p. 71.

wires initially contemplated for the bridge cables, specified that a single wire should sustain an ultimate load of 3,400 lb before breaking. Inasmuch as the approximate diameter of the small wires after galvanizing is 0.165 in., the specified breaking load is equivalent to a unit stress of 159,000 lb per sq in. Only a small quantity of the small-diameter wire had been used, when a change was made to a larger wire with an approximate diameter, after galvanizing, of 0.184 in.

The average unit tensile strength of the wires tested is a little greater than the unit strength indicated in the original specification; thirty-two specimens had strengths greater than 159,000 lb per sq in. whereas twenty-eight

TABLE 5.—PHYSICAL PROPERTIES OF SPECIMENS TESTED IN TENSION

Property[a]	Brooklyn Bridge							George Washington Bridge (Cable Wire)[c]		
	Cable Wire				Stiffening Truss[b]					
	No.[d]	Minimum	Average	Maximum	Minimum	Average	Maximum	Minimum	Average	Maximum
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Ultimate tensile strength	60	140.0	162.6	195.0	69.8	79.0	89.5	220.0	234.0	259.0
Yield strength	24	128.2	143.8	165.5	43.2	48.7	53.8	150.0	184.0	202.0
Proportional limit	24	90.2	99.9	111.6	....	....	....	....	....	....
Modulus of Elasticity:										
8-in. gage length	24	27,400	29,010	32,300	....	....	....	....	....	....
84-in. gage length	11	28,200	28,760	29,500	....	....	....	....	....	....
Percentage elongation	60	0.8	2.88	4.8	18.8	23.2	25.4	....	6.0	....
Reduction in area (%)	60	3.0	15.6	28.8	45.5	49.6	52.4	....	....	....
Diameter, in inches	54	0.1793	0.1841	0.1910	....	....	....	....	0.1965	....
	6[d]	....	0.1653	....	....	....	....	....	....	....

[a] Units are in kips per square inch except where specially noted. [b] Tensile tests of chord coupons. [c] *Transactions*, ASCE, Vol. 97, 1933, Table 8, p. 364. [d] The number of samples tested is given in Col. 2. The six reported in the last line were smaller wires used in a few bottom strands of the cables.

specimens had strengths less than this value. It is of some interest to note that, in general, the bottom wires of the cables exhibited higher strengths than the side and top wires.

The average yield strength of twenty-four samples (Col. 4, Table 5) is 143,800 lb per sq in. This value is 88.4% of the average ultimate strength. The yield strength in these tests is defined as the unit stress at that point on the stress-strain diagram at which the elongation is 0.7%—in other words, the unit strain is 0.007.

The average value for the proportional limit of twenty-four samples is 99,900 lb per sq in. The proportional limit is defined as the unit stress at the point at which there is an observable deviation from the straight part of the stress-strain diagram. This point could be determined more accurately, however, from a study of the variation in increments of strain noted in the tabulated data for each test in combination with a scrutiny of the stress-strain diagram.

The average values for the proportional limit and the yield strength cannot

be compared directly with specification requirements. The original specifications state that the elastic limit (of the small-diameter wire) should be 1,600 lb. This is equivalent to a unit stress of 75,000 lb per sq in. What was meant at that time by the elastic limit is not clearly defined in the specifications.

The modulus of elasticity was found to have an average value of 28,760,000 lb per sq in. This value is based on stress-strain diagrams for eleven of the twelve tests of long samples using an 84-in. gage length. One test was discarded because the wire was badly corroded. The original specifications required that values for the modulus of elasticity should fall between 27,000,000 lb per sq in. and 29,000,000 lb per sq in. Values for E were also determined from twenty-four tests of short samples, using a gage length of 8 in. The average for this series is 29,010,000 lb per sq in. However, in one of the tests averaged, the modulus was determined as 32,300,000 lb per sq in., which was considerably "out of line" with the values for the remaining twenty-three tests. Omitting this value, the average was reduced to 28,870,000 lb per sq in. The average values for the short-gage tests and the long-gage tests show very good agreement.

The average value for ultimate elongation in 10 in., based on sixty tests, is 2.88%; twenty-five samples had elongations in the interval between 2.0% and 3.0%; and twenty four were in the interval between 3.0% and 4.0%. The original specifications (for the small-diameter wire) called for a minimum elongation of 2% in a 50-ft gage length or 3.5% in a 5-ft gage length.

A direct comparison of test values for elongation with original specified values is not possible because of the difference in gage lengths. However, the wire seems somewhat deficient in ductility. It is of some interest to compare it in this respect to modern cold-drawn suspension bridge wire. The wire for the George Washington Bridge (Cols. 9 to 11, Table 5) was required to have a minimum elongation of 4% in 10 in., but the average value of 26,274 tests[17] was 6.0%. Moreover, the average tensile strength of the cable wires in the George Washington Bridge was 234,000 lb per sq in., which is considerably higher than the average tensile strength for wire in the Brooklyn Bridge. It is important to remember that, in general, ductility decreases as ultimate strength increases.

The average value for reduction in area at the point of fracture is 15.6%. The original specifications for the small-diameter wire stated that necking at the break should be such as to give a diameter less than 0.15 in. Since the original diameter of this wire was approximately 0.165 in., this statement is equivalent to specifying a minimum reduction in area of 17.5%. The significance of "reduction in area" is a subject for considerable debate. There is no definite correlation evident in these tests between reduction in area and elongation or tensile strength.

A comparison of ultimate tensile strengths with carbon content, determined from chemical analyses of twelve samples indicates that, in general, strength increases as the carbon content increases. The carbon ranged from 0.55% to

[17] *Transactions*, ASCE, Vol. 97, 1933, p. 364.

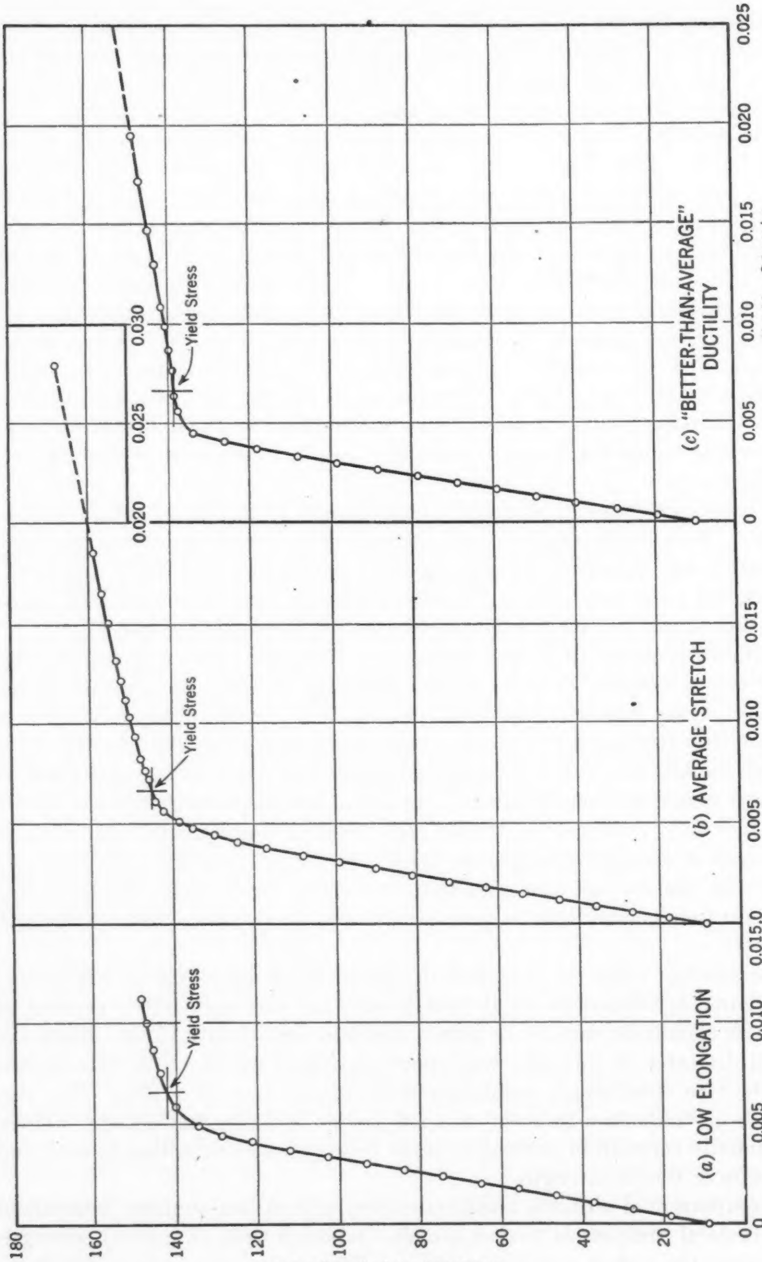

FIG. 29.—TYPICAL STRESS-STRAIN DIAGRAMS FOR CABLE WIRE (SEE TABLE 6)

0.89% whereas the tensile strength for the twelve samples ranged from 143,600 lb per sq in. to 184,000 lb per sq in., the detailed comparison being as follows:

Ultimate strength (kips per sq in.)	Carbon (%)
143.6	0.63
144.6	0.55
151.0	0.68
154.0	0.63
157.8 (Fig. 29(*c*))	0.67
162.2	0.71
166.7	0.74
167.4 (Fig. 29(*b*))	0.76
172.0	0.70
174.0	0.82
178.5	0.89
184.0	0.84

Typical stress-strain diagrams for the tensile tests are shown in Fig. 29. The physical properties accompanying these diagrams are listed in Table 6.

TABLE 6.—CHARACTERISTICS OF SPECIMENS PLOTTED IN FIG. 29

Characteristic	Fig. 29(*a*)	Fig. 29(*b*)	Fig. 29(*c*)
Ultimate tensile strength (lb per sq in.)	148,500	167,400	157,800
Yield strength (lb per sq in.)	142,500	144,800	138,000
Proportional limit (lb per sq in.)	102,900	99,000	98,000
Modulus of elasticity (lb per sq in.)	28,700,000	28,800,000	29,500,000
Percentage elongation in 10 in.	1.1	2.8	3.2
Percentage reduction in area	0	17.4	13.9

Fig. 29(*a*) pictures the data for a specimen having low elongation; Fig. 29(*b*), for one with average stretch, and Fig. 29(*c*), for one with "better-than-average" ductility. All the stress-strain curves show a flat slope above the yield point. In general, the reserve of strength between the yield stress and the ultimate strength is small. The diagrams also exhibit a sharp increase in curvature or "knee" in the transition zone from elastic to plastic behavior. Although the yield strength is a value rather arbitrarily defined in cable-wire tests, nevertheless, in these tests, it seems to coincide rather closely with what might be termed a point or region where yield begins to be pronounced.

Bend tests on samples of the wire from which the galvanizing had been removed were conducted by the New York City Department of Public Works in its own laboratory. The detailed results of those tests are not included in this paper. The results indicated that the wire was deficient in flexibility. More than one half of the samples tested could not be coiled continuously (at least one 360° turn) around a cylindrical mandrel less than 1 in. in diameter. There was practically no correlation between the results of these tests and the elongation obtained in the tensile tests. On the other hand, some correlation was evident between the bend data and the values for reduction in area. In general, the wires that broke on the large-diameter mandrels (1 in. and 1.5 in.) gave small reductions in area on the tensile tests.

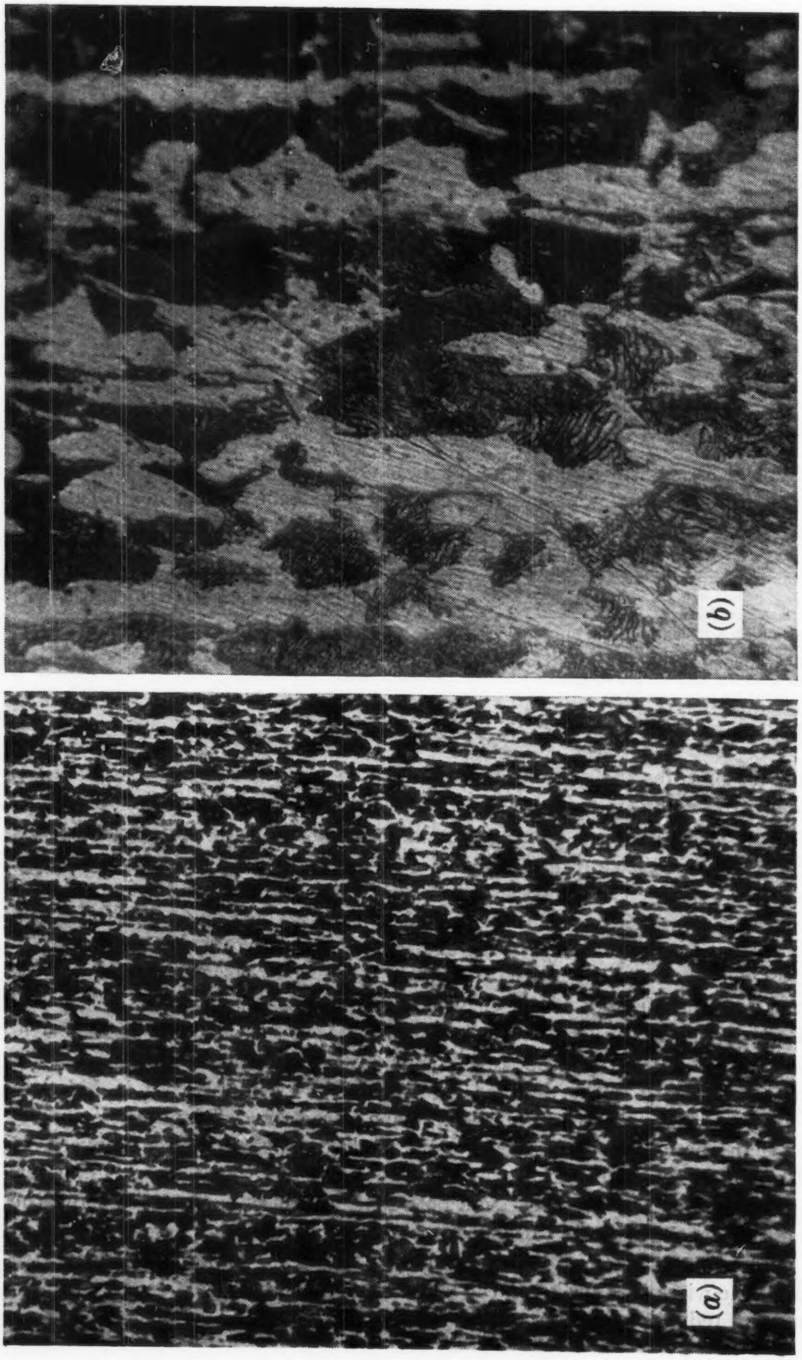

FIG. 30.—WIRE SAMPLE WITH BANDED STRUCTURE
(a) MAGNIFICATION 100 ×

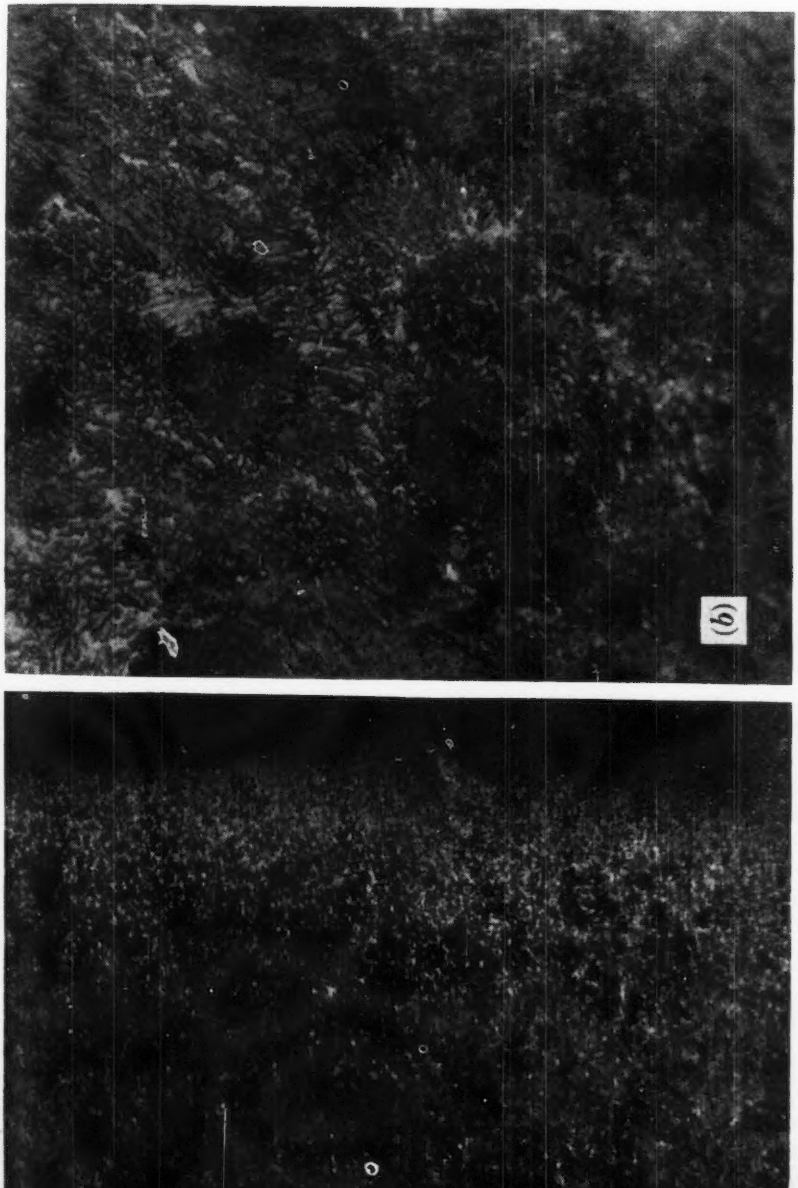

Fig. 31.—Photomicrographs Showing Perlite Structure
(a) Magnification 100 ×
(b) Magnification 600 ×

"Scleroscope" hardness tests were made on twenty-four samples. The hardness ranged from 55 to 66 except for one very soft spot on one sample. In general, as hardness increased, tensile strength increased.

The metallographic examination of twenty-four samples of cable wire showed variations and irregularities that would not be encountered in wires made more recently, under rigid, modern standards of inspection. It must be remembered that these wires were made at a time when it was the exception rather than the rule to make analyses of steels. Very few steel plants at the time were equipped with chemical laboratories, and metallographic examination was still unknown.

Most of the wires seem to be a good grade of steel, of eutectoid (0.83% *C*) composition, approximately. However, two wires were of hypereutectoid structure, five were decidedly hypoeutectoid, and three showed a banded structure like that illustrated, in Fig. 30. The same sample is shown in both Figs. 30(*a*) and 30(*b*), but Fig. 30(*a*) was originally taken at a magnification of 100 diameters; and Fig. 30(*b*), at 500 diameters. This banded structure is generally considered indicative of poor quality and low strength. The white streaks are ferrite, practically pure iron, and the dark areas are perlite, an iron-iron carbide mixture.

Phosphorus, silicon, and other elements present in steel sometimes remain segregated, even after rolling. They become elongated in the direction of hot working. Carbon is less soluble in these areas than in the regions of lower impurity content and the result is the banded structure in Fig. 30. Banding gives directional properties to steel.

The photomicrograph in Fig. 31(*a*) shows a sample of better wire typical of most of the wires, under a magnification of 100 diameters. Fig. 31(*b*) is the same sample magnified 600 diameters. This particular sample is a little higher in carbon than most of those tested but, nevertheless, illustrates the eutectoid composition. The structure is almost entirely of perlite, as evidenced by the predominance of dark areas. Fig. 31(*a*) shows, however, appreciable decarburization along the outer surface of the wire. A short sliver of steel should be noticeable at the left center extending into the zinc coating (black).

In one respect—namely, in the structure of the perlite—all the wires examined were alike and of high quality. The individual grains were small, and the laminations within the grains were fine. Both these characteristics tend to improve the strength of the wire—other factors being the same, the smaller the grain size, the greater the tensile strength. Otherwise, every feature that could be observed under the microscope varied from wire to wire.

Although some of the wires were as free of nonmetallic inclusions as wire produced today, many of the samples had far too many slag and oxide inclusions. The banded structure shown by several of the wires indicated not only hot rolling but also too high a percentage of undesirable constituents such as phosphorus, sulfur, and slag.

Several of the samples were nonuniform in the distribution of the carbon. This variation appeared in two independent forms. In one case, isolated spots of relatively carbon-free ferrite of appreciable size were scattered throughout a matrix of essentially 0.8% carbon. In the other case, which is more serious, the

wires show a widely varying amount of decarburization along the surface. This decarburization will give the wire a lower tensile strength than it would otherwise have had.

The technical press has recorded[18,19,20] that the original small-diameter wire was cold-drawn through two dies from rolled crucible rod steel, oval in section, with diameters of approximately 7/32 in. by 15/64 in. The first drawing reduced the wire to No. 7 gage and the second drawing to No. 8 gage, the latter being approximately 0.165 in. in diameter. Only a relatively little of the smaller wire had been used when the change to a diameter of about 0.184 in. was made, which was slightly less than that of No. 6 wire. Available literature offers no evidence that the larger wire was still given two drawings and that the size of the basic crucible rod steel was increased.

From the microscopic examination of the samples it would appear that only a few of the wires were given a real wire-drawing operation. This lack of wire drawing is indicated in two independent ways: (1) The longitudinal section of cold-drawn material shows the grains, especially near the surface, to be drawn out axially; and (2) wire drawing produces a smooth surface. Most of the samples showed no axial tendency at all; and, in cases where it would be observed, such a tendency was restricted to a very superficial depth. If the hole in the die is not perfectly circular, ridges extending along the wire may be produced; but, even so, the cross-sectional shape does not vary from place to place. Many of the wires examined were extremely rough, the high and low spots occurring in random order. That this was the condition of the wire at the time of manufacture is shown by the way in which the zinc coating has filled in these irregularities.

The greatest irregularities were noted in the zinc coating. Some samples showed a heavy, uniform coating; others, a very thin uniform coating; and in

TABLE 7.—CHEMICAL ANALYSES OF CABLE WIRE (PERCENTAGES)

Description	Brooklyn Bridge					George Washington Bridge[a]				
	Carbon	Manganese	Phosphorus	Sulfur	Silicon	Carbon	Manganese	Phosphorus	Sulfur	Silicon
Minimum..	0.55	0.30	0.076	0.038	0.16	0.72	0.44	0.021	0.022	0.07
Average...	0.75	0.38	0.099	0.052	0.19	0.81	0.63	0.029	0.034	0.19
Maximum.	0.91	0.40	0.128	0.067	0.23	0.93	0.77	0.042	0.046	0.34

[a] *Transactions*, ASCE, Vol. 97, 1933, Table 7, p. 362.

some the coating was extremely irregular. The irregularity may have been due, in part at least, to the flow of the zinc occurring under pressure when the individual strands were pressed into the form of the cable and then wrapped.

The results of the chemical analyses of the cable wire are compared with similar analyses for George Washington Bridge[21] in Table 7. Carbon,

[18] "East River Bridge Wire," *The Iron Age*, January 4, 1877, p. 5.

[19] "Making Wire for the Brooklyn Bridge," *ibid.*, March 1, 1877, p. 1.

[20] "Wire for the East River Bridge," *Scientific American*, March 3, 1877, pp. 127 and 130.

[21] *Transactions*, ASCE, Vol. 97, 1933, Table 7, p. 362.

phosphorus, and sulfur contents were determined in twenty-four samples, whereas manganese and silicon contents were found for four samples. The wide range of carbon content is apparent. In the cable of the George Washington Bridge, the range for 958 melts of steel was from 0.72 to 0.93, with an average of 0.81. The phosphorus and sulfur contents are considerably higher than those in modern bridge wire which generally average less than 0.04%.

It is not within the scope of this paper to recommend a working stress to be used as a basis in evaluating the Brooklyn Bridge cables under modern loading conditions. Nevertheless, it is advisable to call attention to a number of factors evident from the physical, chemical, and metallographic studies which have a bearing on this question. The stress-strain diagrams show a rather flat slope above the yield strength. There is not much difference between the yield strength and the ultimate strength. The elongation is low. Even though the ultimate strength of this wire is about 70% of that for modern bridge wire, the energy capacity is less than 50% of the energy capacity obtained today.

The carbon content exhibits a wide range. The metallographic studies corroborate this conclusion and also indicate nonuniformity in other characteristics. The galvanizing coat is quite variable in thickness. In view of all these factors, it seems wise to be conservative in establishing a limiting working stress for any revision in loading conditions.

This statement is not to be construed as indicating a belief by the writer that the wire is definitely inferior in quality. Such is not the case. It must be remembered that this wire was manufactured more than 60 years ago, when standards for insuring uniformity were not as definitely established as they are today. The cable wire has stood the test of many years of satisfactory service and its quality today is high enough to warrant the expectation of many years of added service.

Chord Samples from Stiffening Trusses

Sixteen flat coupons were machined from steel samples cut from the chords of the stiffening trusses of the Brooklyn Bridge. The results of the physical tests made on these specimens are summarized in Table 5, Cols. 6 to 8.

The average value for the ultimate strength is 79,000 lb per sq in. The average value for the yield point is 48,700 lb per sq in. These values are somewhat higher than those generally encountered today in low-carbon structural steel as defined by Specification A7–42 of the American Society for Testing Materials for bridges and buildings.

The average value for ultimate elongation for all sixteen tests is 23.2%. The average for the eight samples with 8-in. gage is 23.1% whereas the average for the eight samples with 7-in. gage length is 23.2%. These values are practically the same as values obtained today in tests of low-carbon steel.

A metallographic study and a chemical analysis were made for two of the samples, the one with lowest strength (69,800 lb per sq in.) and the one with highest strength (89,500 lb per sq in.). The low-strength sample had a carbon content of 0.17% and a manganese content of 0.68%. The high-strength specimen had a carbon content of 0.26% and a manganese content of 1.43%.

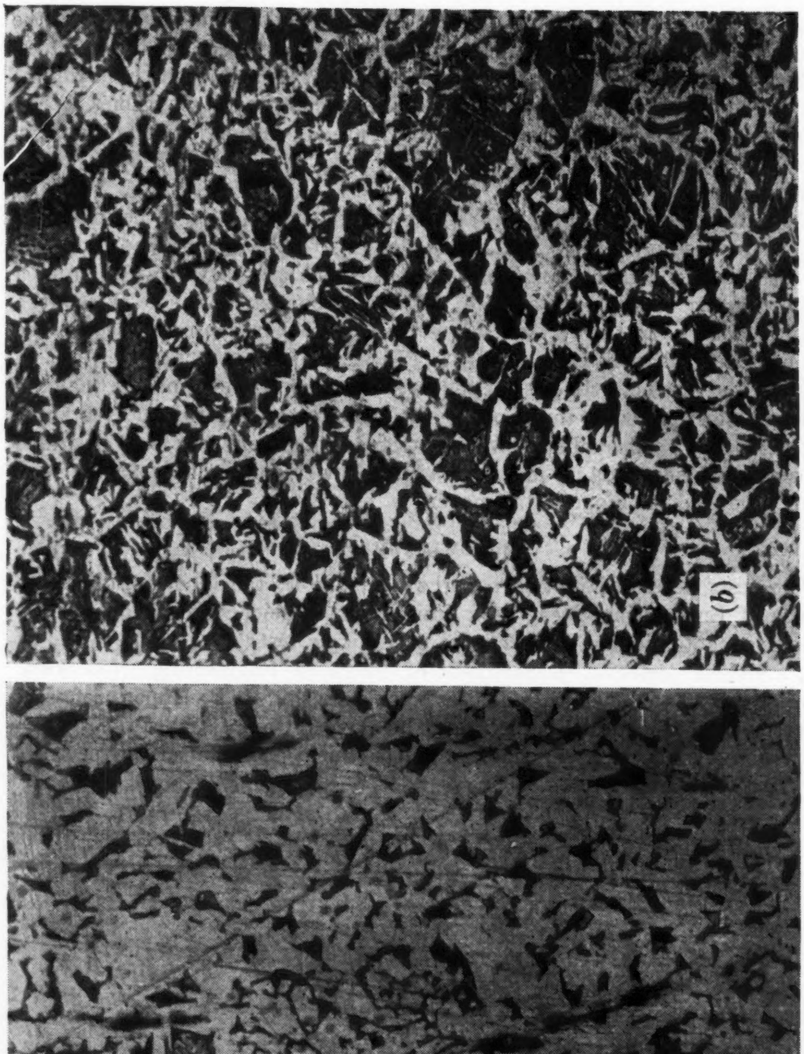

Fig. 32.—Photomicrographs of Chord Specimens
(a) Low-Strength Sample at 100 ×
(b) High-Strength Sample at 100 ×

The difference in the quantities of these elements undoubtedly was one of the factors accounting for the difference in ultimate strengths of the two samples.

Fig. 32(*a*) is a photomicrograph of the low-strength sample with a magnification of 100 diameters. Fig. 32(*b*) shows the high-strength specimen. When compared with Figs. 30(*a*) and 31(*a*), which are also magnified 100 diameters, these views show clearly the larger grain structure associated with lower-strength steels.

As is to be expected, Fig: 32(*b*) shows a higher proportion of perlite (dark area) than does Fig. 32(*a*). This is due, not only to the higher carbon content, but also to the higher manganese content which places this steel, from a metallographic standpoint, outside the low-carbon steel class. Without a chemical analysis of such a steel, an estimate of carbon content based on the area of perlite might be considerably in error.

From the sixteen tests of steel coupons, it seems safe to conclude that the structural steel placed in the Brooklyn Bridge trusses more than 60 years ago is of unusually good quality. Whether the sections have the most desirable shape and sufficient area for modern loadings, however, is another matter—one which is beyond the scope of this paper.

Acknowledgment

The tests described in this paper were made at the laboratories of the College of Engineering, New York University. The metallographic studies (including photomicrographs) and the galvanizing tests were made by F. C. Fair, assistant professor, Department of Chemical Engineering, New York University; and the chemical analyses were made by H. B. Hope, professor, Department of Chemical Engineering, The Cooper Union—under the general supervision of H. J. Masson, chairman, Department of Chemical Engineering, New York University.

AMERICAN SOCIETY OF CIVIL ENGINEERS
Founded November 5, 1852

REPORTS

EVALUATION OF PROFESSIONAL OBJECTIVES IN THE DESIGN OF SANITARY ENGINEERING WORKS

REPORT OF A COMMITTEE OF THE SANITARY ENGINEERING DIVISION

I. Origin and Purposes of Committee

The Committee on Evaluation of Professional Objectives in the Design of Sanitary Engineering Works was appointed by the Executive Committee of the Sanitary Engineering Division in January, 1944. As defined by the Executive Committee,

> "The purpose of this committee is to evaluate the professional objectives of engineering in designing water purification and sewage treatment works with reference to the influences of proprietary equipment and processes upon the development of effective and economical plans that will best meet the need of the community or industry to be served."

In further explanation of the assignment to the new committee, the Executive Committee stated that:

> "The situation existing at this time in the purification of water and sewage, due to the many processes that are being used, many of which are covered by proprietary patents and actively promoted commercially, has caused serious problems in obtaining designs that will at the same time provide sufficient competitive conditions to assure lowest costs to owner. * * * In its investigations, the committee may properly give consideration to relative effectiveness of different processes, conditions where certain of them would be especially well adapted; and any other features having not only direct but collateral bearing upon its recommendations and suggestions leading to a more intelligent handling of water purification and sewage disposal from the standpoint of effective service to the public by the engineering profession."

II. The Situation

During the past decade a number of process patents relating to the treatment of water and sewage have been issued and are being actively promoted commercially by the assignees, many of whom are in the business of manufacturing and selling plant equipment. The promotion of patented articles

Note.—Written comments are invited for immediate publication; to insure publication the last discussion should be submitted by June 1, 1946. This Report is published in *Proceedings* only.

and processes in the field of water and sewage treatment is not new, of course; but the practice has grown to such an extent recently that it threatens to interfere with conventional practice in the design of sanitary engineering works. Significant facts that have been brought to the attention of the Committee are as follows:

a. In the design of water and sewage treatment plants for municipalities, it is customary for the designing engineer to specify that the contractor shall assume liability for the payment of royalties or for patent infringement. The procedure is intended as a protection for the client, but it is in fact an avoidance of the question of patents by the engineer. In the past this practice has not interfered with adequate competition in bidding, probably for the reason that the claims for the payment of royalties or for infringement have not been numerous. In recent years, however, the patents involved and the threats of claims for infringement have become so numerous as to demand recognition by the designing engineer. He is thus more or less restricted in design since he has no means of knowing what may be covered by patents or whether or not claims of infringement may be made.

b. As the designing engineer becomes aware of the coverage of a patent through the promotional activities of manufacturer's agents, he is faced with the responsibility of deciding whether: (1) To honor the patents and thereby to subject his client to the payment of royalties and to a restriction of competition in bidding; (2) to infringe the patent and thereby to subject his client to the possibility of expensive patent litigation; or (3) to avoid the patent by changes in process or in design. Indications are that the latter course has been the least troublesome to both client and designing engineer, and it has usually been followed where possible. When it is followed, it may deprive the client of the use of a valuable process or piece of equipment. In recent years the designing engineer has sometimes found that his efforts to avoid infringement by changes in design have been to no avail because the changes themselves have been covered by patent applications not yet made public.

c. Many of the patents brought to the attention of the designing engineer appear to him to be of doubtful validity. Even though the process or equipment covered by the claims of such a patent may be desired for his client, he is loath to subject his client to the payment of a royalty therefor until he is sure of the standing of the patent. The validity of a patent is not established until the patent is upheld in an infringement suit. Most of the patents of today have never been subjected to infringement litigation. Nevertheless, it is the duty of the engineer to his client to form an opinion as to the validity of such a patent.

d. Some of the process patents obtained in recent years appear to be restrictions on established practice in that they purport to cover combinations of processes which have been previously used, or to establish limits on certain design factors or rates that may be used without infringement.

e. Licenses for the use of certain patented processes are not usually readily obtainable by direct purchase, but are furnished to purchasers of equipment. This practice tends to force the use of equipment made by one manufacturer

and thus to remove these articles from competitive bidding, even though equivalent equipment may be available.

f. In some cases, patent claims have been construed and interpreted to bidders in such a way as to restrict competition on plant equipment which would normally be considered as having no connection with the patented process.

g. Some of the manufacturers who are actively promoting patented processes have entered the engineering field and are offering free engineering services. These services are being advertised widely in technical magazines in connection with the processes, and are available to the engineer or to the client directly. Such services are obviously of value in connection with the process itself; but they are necessarily biased and, when accepted by a client directly, competition is automatically eliminated. Since the design of a treatment plant involves a great deal more engineering than that connected with the process or equipment sold by a single manufacturer, there is danger of an unsuccessful or uneconomical plant in a case where the client deals directly with the manufacturer.

h. Some engineers who have been responsible for the design of water works or sewage treatment works have adopted, as their own, plans and specifications prepared by promoters of patented processes or equipment. It is reported also that some engineers have resorted to the practice of selecting methods of treatment on the basis of whether the manufacturer would furnish the working drawings for the completed design.

i. Some of the manufacturers who are promoting patented processes actively have made claims of efficiency or economy for their processes which have subsequently been found to be exaggerated. Operating data represented to be typical have been found, upon investigation, to have been collected over short periods of time and under particularly favorable circumstances. Construction cost estimates, upon examination, have been found to have been based upon unit costs far below prevailing bid prices.

III. Scope of the Committee's Work

The Committee has had one all-day meeting and the members have conferred and corresponded freely during the past two years. One of its first tasks was to agree upon the scope of its activities and to attempt to set definite objectives.

A progress report was submitted at the January, 1945, Annual Meeting of the Society, a copy of which is presented in the Appendix.

The problems to be studied involve three groups of interests—the Client, the Engineer, and the Promoter of proprietary processes or equipment. For the recommendations of the Committee to be effective, they should be of help to all three groups in overcoming the existing difficulties. The interests of these three groups have been defined by the Committee as follows:

1. The Client, usually a public agency but sometimes an industry or person, desires the best available solution of a problem at a minimum of expense and inconvenience. He also desires the benefits of new processes or equipment

as early as practicable. The client desires the right to purchase the solution of his problem in an open market and to use his own judgment as to procedure, except as limited by law.

2. The professional Engineer desires to be engaged by the Client to study the problem from the beginning, because he believes that he, through his knowledge and experience, is in a position to make unbiased examination of available processes and equipment and to give competent advice on their selection as well as to combine all units into a completed workable and economical plant.

3. The Promoter of proprietary processes or equipment, usually a manufacturer, is in the business of selling his particular process or apparatus. To meet competition he must improve existing products and develop new ones. In order to protect his investment in the development and promotion of a new product, letters patent are essential; and, in order to sell a new product, advertising and promotion are essential.

In view of the foregoing statement of interest, the Committee construes that its objective is as follows:

> To describe clearly the position and the necessary restrictions of the three groups in order to bring about a more satisfactory relationship between the Client, the professional Engineer, and the Promoter to the end that all may benefit.

Since the Committee is a creature of the Society its recommendations are necessarily directed to the members of the Society, and the burden of bringing about better relationships rests upon the Society membership.

The Committee believes that some of the matters suggested for its consideration are not proper tasks for the Society, and the Committee has excluded these matters from its work.

For example, the Committee cannot study the relative effectiveness of different processes. Such a study would have value only if applied to a specific project, and it is the function of the engineer on each such project to make this study. This, in fact, is the primary reason for the employment of an impartial professional engineer on a project involving the use of water treatment or sewage treatment processes.

The Committee cannot attempt to pass upon the validity of patents or of patent claims. This is a matter for the courts. The Committee might make itself and the Society liable to suits for damages if it should publish an adverse opinion on a patent or patented process. There is no point in passing upon the validity of a patent unless the process or equipment covered thereby is desired by a particular client. In this case, the client may properly be advised by his engineer or his attorney relative to the value of the patent.

IV. Trends in Engineering Practice in Dealing with Patented Equipment and Processes

Many public agencies are required by law to award contracts in excess of $500 to the "lowest responsible bidder." Statutes requiring competitive bidding have been adopted to insure economy and to promote honesty and good public administration. Contracts for construction of public water and

sewage works projects are commonly let on the basis of plans, specifications, and contract documents prepared by engineers engaged for that purpose by the public agency. Because of the requirement that the engineer should act as an unbiased adviser to the public agency, it has been considered good professional ethics that he should have no financial interest in any of the contracting firms that tender bids or in any article or process specified.

According to Nelson Rosenbaum,[1] the procurement of a patented article or process by competitive bidding has been under judicial consideration for three quarters of a century. The problem arises because bidders are required to submit proposals on equal terms, usually in response to definite specifications. This equality obviously cannot exist when the article or process specified is patented, since the patentee has "an exclusive right to make, use and vend" the device or process claimed in the patent.

Because generally there is legal compulsion for competitive bidding, public officials in some jurisdictions have been unable to obtain patented articles or processes. In such jurisdictions, the "Wisconsin" or "exclusion" rule is said to apply. In other jurisdictions, statutes requiring competitive bidding have been interpreted as not applying to patented processes or articles, in which case the "Michigan" or "permissive" rule is said to apply. In order to secure some competition with patented articles, two additional rules known as the "equal opportunity" and the "alternate" rules have been adopted in some states. Under the "equal opportunity" plan, all bidders have an equal opportunity to secure the patented portions of the specifications upon the same terms from the patent holder. Under the "alternate" rule, alternate bids are taken on several articles or processes serving the same purpose, including one or more patented articles or processes.

The right to specify a patented article does not authorize a public agency to include it in one contract with work which is unpatented and capable of separate performance and for which competition can be secured. A contract so awarded is not valid even though competition may be obtained in the bidding. Furthermore, no insurmountable barrier exists to the acquisition of patented articles by public bodies. Any judicial or statutory impediments can be removed by appropriate remedial statutes, as the source of the problem is legislative.[1]

The rapid mechanization of processes in the field of water treatment and sewage treatment has brought with it the use of numerous pieces of mechanical equipment, many of which are patented. The practice has developed, among engineers for public agencies, of specifying such mechanical equipment in general terms without mentioning the trade name of the patented article; or, if the trade name is mentioned, by adding the words "or equal." In most contracts for public agencies, both the engineer and client seek to avoid liability for infringement of patents by throwing the responsibility on the contractor. This practice has doubtful legal standing. It is practically impossible today to avoid the use of patented articles in a modern treatment plant even in jurisdictions where the exclusion rule applies. In this respect, practice has outstripped the law, and there is a tendency among engineers to differentiate

[1] "How Cities Buy Patented Articles," by Nelson Rosenbaum, *American City*, June, 1944, p. 85 and July, 1944, p. 89.

between patented articles and patented processes on the dubious grounds that the former are unavoidable but the latter restrict competition on public works. Royalties for patented articles are easily hidden in the purchase price, but royalties for a patented process must be paid directly for the process or be included in the purchase price of unpatented articles associated with the process.

The legal compulsion for competitive bidding does not apply to private persons or to industries who contract to purchase water treatment and sewage treatment plants. Although some competition is usually good business for such clients, they are, nevertheless, not restrained from purchasing patented processes and equipment. The need for an unprejudiced engineer as a third party to advise in the selection of processes and equipment for industrial clients is therefore not so evident as in the case of public agencies, and many such clients purchase directly from manufacturers, the latter furnishing engineering service as well. This method of doing business is usually not the most economical, but it has the advantage of throwing the full responsibility for plant performance on the manufacturer.

Thus, there are two fields of practice for sanitary engineers in designing water treatment and sewage treatment plants which are characterized by two distinctly different types of clients, one the public agency and the other private industry. The public agency field is much the older and larger, and most sanitary engineers practice in this field. Such engineers for the most part have a civil engineering background, and they have in general avoided connections with patented processes or equipment in order to maintain their unprejudiced status. On the other hand, the rapid industrial development in the United States has brought with it an increasing demand by industry for treated water and treatment of industrial wastes with a corresponding increase in opportunity for sanitary engineers. Practice in the industrial field has tended toward the elimination of the disinterested professional engineer and the extensive use of patents to protect new developments. Manufacturers engaged in the promotion and sale of water treatment and sewage treatment plant equipment have been driven by competition to the development of new processes and devices, and they have employed sanitary engineers for this work. Such engineers, although necessarily biased in favor of their products, are on the whole as competent technically as the disinterested professional engineer serving public agencies.

It is pertinent to point out in this connection that this trend in engineering practice has developed much more extensively in the mechanical, electrical, and chemical fields. A great majority of engineers in these fields are employed directly by proprietary interests, and disinterested consultants are relatively very scarce. The rapid technical and scientific progress in these three fields raises the important question as to whether the disadvantages of the system "Contractor direct to Client" outweigh its merits. Whatever the answer, the system is strongly entrenched in private business and is now being extended to public business.

A further side light on engineering trends with references to patents appears in the proposed "Canons of Ethics for Engineers," as prepared by the Engineers Council for Professional Development. Section 16 of this document states

that the engineer:

> "* * * will make his status clearly understood to his Client or employer before undertaking an engagement if he may be called upon to decide on the use of inventions, apparatus, or any other thing in which he may have a financial interest."

Note that in this code, the engineer is not called upon to avoid financial connections with proprietary processes and equipment but merely to state his position to his client.

A good deal of testing of processes and equipment has been undertaken by engineers and chemists of public health agencies. Since the public health agencies generally have the power of approval of plans for new water works and sewage works for municipalities in order to insure that they will function properly, it has been necessary for public health authorities to advise on types of treatment and to assist engineers in the testing of processes. Rules and regulations have been adopted by many public health authorities which fix arbitrary limits on such design factors as filter rates, detention periods, etc. These rules, if interpreted rigidly, interfere with the adaptation of a process to fit a particular case, and they tend to reduce the consulting engineer from the status of a "process designer" to that of a "plant designer." If these regulations are necessary, they are evidence that there are too many engineers in practice who are not sufficiently expert in the selection and design of water treatment and sewage treatment processes.

In some cases, engineers serving public agencies who have successfully developed new processes or devices have sought to "protect" the public's right to the use of such processes or devices either by not patenting them or by assigning the patents to the public. Although this procedure does permit free use of the new development by the public, the public does not use it extensively because it does not learn about it. The elimination of the proprietary interest also eliminates the advertising and promotion which is essential to place the new development before the public.

In recent years proprietary interests in the fields of water treatment and sewage treatment have sought aggressively to increase their business with public agencies. Competition has been keen and new developments have been rapid. In efforts to minimize competition, some companies have made ingenious and sometimes questionable use of patents. For example, a number of patents have been issued on combinations of old processes with new ones, thus restricting the use of old processes. Other patents have been issued which restrict the free use of old processes to narrow operating limits. Moreover, some promoters have allegedly made claims for their patents which appear to be beyond the scope of the patents. These practices have tended to reduce competition, but they have also served to antagonize public agencies and their engineers and have probably delayed the adoption by the public agencies of worthwhile new processes and equipment.

Licenses for the use of patented processes are not generally obtainable by direct purchase at the present time but are furnished to purchasers of equipment. This inability of the client to purchase a license directly limits, automatically, the choice of plant equipment to that manufactured or sold by the

patent assignee. However, recent interpretations of patent law seem to indicate that the owner of a patented process cannot protect the sale of unpatented articles or equipment for use in the patented process. It is possible therefore that an informed client may be able to force the sale of a license to use a patented process without having to purchase unpatented equipment from the assignee of the patented process.

It is not good engineering to incorporate, in plant design, processes or equipment, patented or otherwise, unless they have proven merit. The question arises as to what constitutes "proven merit." Many engineers adopt the practice of not using new processes or devices until they have been used elsewhere over a period of years. It is obvious that, if this were the exclusive practice, the public would be denied the right to all new developments.

In the sanitary engineering field, there have been sporadic attempts at development or testing of new processes by engineers for public agencies. These experiments have generally been on a fairly large scale due to the widespread belief that plant conditions cannot be duplicated in the laboratory or by pilot plants. Some progress has been made; but, because of the time and expense entailed, it has not become general practice in the sanitary engineering field to submit proprietary processes and equipment to test. One prominent engineer states that,

> "It has been my observation that even when Promoters have faith in their processes and agree to a test, the result is very frequently a stalemate because the sewage was different than contemplated, the plant arrangement was not suitable or some other condition was not right. One has only to review in his mind the succession of 'new' processes that have appeared on the horizon since beginning with direct oxidation to recall that most of the trial tests were quite unsatisfactory in the way of furnishing unbiased and reliable data."

This situation is in marked contrast with other industries, particularly the chemical industry, where it is now quite general practice to invest vast sums in new plants involving some new processes or equipment which have been developed by laboratory and pilot-plant tests. Science and engineering practice have now developed to the stage where it is quite feasible to "prove" a process or piece of equipment by laboratory or pilot-plant methods.

In recent years much development and testing of processes and equipment for water treatment and sewage treatment have been done by proprietary interests. Thus, performance data have been collected which have been used by the manufacturer in advertising and selling his process and equipment. This method of procedure has merit in that new devices and processes are made available to the public. On the other hand, public agencies and their engineers are loath to accept these data as satisfactory evidence of performance since the data are necessarily biased.

V. Patents and Patent Claims

A patent is a paper issued by the government on a patentable invention which grants to the patentee a seventeen-year monopoly on the invention from the date of issue of the patent. This monopoly is granted in return for making

the invention public, and the letters patent are the device by which the invention is publicized.

An invention is patentable only if it contains something novel and useful. By its very nature, a patentable invention brings into being a natural monopoly which merely requires secrecy for its preservation. The commercialization of many inventions, such as those for water treatment and sewage treatment, would destroy the secret. Hence, patents are necessary not only to protect the monopoly but also to insure that the public gets the benefit of the invention as soon as possible.

A Patent consists of two parts—(1) the Specification in which the applicant describes the nature and novelty of his invention and its utility, and (2) the Claims in which the Applicant makes one or more specific claims of novelty in the art on which he desires a monopoly. In applying to the Patent Office, the inventor (usually through his patent attorney) files a Patent Application, which, with subsequent changes required by the Examiner, becomes the Patent itself, if and when it is granted.

It is the responsibility of the Examiner to search for "prior art" in the patent files and to cite patents or court decisions which in his opinion tend to deny the novelty or validity of the invention. In an Action, he then notifies the Applicant of denial of Claims with reasons for each denial. An Amendment is then made of the application in which the Claims are changed as required, or the Applicant submits arguments for not changing them. Several such Actions and Amendments may be made over several years before the Patent Office finally rejects or allows a Patent.

The date of an invention, upon which hinges the question of prior art and hence the validity of one or more claims, is the date on which the invention was conceived. Since, in case of litigation, the inventor may be called upon to establish the date of invention, documentary evidence of some sort is desirable. The most obvious evidence is the patent application itself, but in many cases there are documents with prior dates. If a patent is granted the monopoly extends from the date of invention through the date of issue and for a seventeen-year period thereafter, unless the patent is declared invalid in a court action.

It is not necessary for an inventor to demonstrate by experiments to the examiner that a new process or device will perform as described in the application. His claims must appear reasonable to the examiner, but the latter is mainly concerned with the novelty and utility of the invention. Many patents are granted, therefore, on processes or devices which when subsequently tested are found not to perform as claimed. This is an appropriate procedure since protection is granted during the developmental period. An engineer or client should not assume, however, that because an article or process is patented that performance will be in accordance with the letters patent.

Although it is the purpose of a patent to grant a monopoly, the only rights actually granted are the rights to sue infringers. The value of the claims are not established until proved valid in a court of competent jurisdiction.

Many of the advances which have been made in water treatment and sewage treatment have come through the protection of patents. The research,

development, and promotional work costs money. Unless patents can be used to protect the chance for profit, there will be no incentive for progressive research. Patents are used for this purpose in every field of endeavor. They are the backbone of progress. It is the opinion of the Committee that patents should be recognized and honored by sanitary engineers until unmistakable evidence is available that they are not valid. Unfortunately, the law is such that a patent can be brought to the attention of a court only by means of an alleged infringement followed by suit on the part of the patentee. It is the duty of engineers therefore not to condone or support infringements unless the proposed infringement is preceded by a thorough legal examination of the patent, a thorough engineering study of the process, or both, and unless these investigations indicate clearly that a court test is desirable for the protection of the public.

An infringement suit is an expensive procedure which is avoided by proprietary interests if practicable. One method of avoiding such suits is to ignore infringers if not too numerous or to grant courtesy licenses to them. Another method is to extend licenses to competitors who might otherwise infringe. The practice of exchanging licenses is becoming more common among the proprietary interests dealing in processes and equipment for water treatment and sewage treatment. This is a desirable trend in that it promotes more competition in public lettings.

Engineers for public agencies have complained that in some cases patent claims have been so construed and have been so represented to bidders that competition has been restricted on plant equipment that would usually be considered as having no connection with the patented process. The remedy for this, in the opinion of the Committee, is for the client to assume full liability for infringement. As has been stated previously, the practice of specifying that the contractor should assume infringement liability has doubtful legal standing at best. The bidder is usually required to submit his proposal within a few weeks after receiving access to plans and specifications. If the plans involve new processes or equipment, it is obvious that there is insufficient time for him to evaluate the liability of infringement.

VI. Legal Requirements for Engagement of Engineering Services by Municipalities

In the deliberations of the Committee the question arose as to whether engineers were free to set their fees high enough to permit thorough analysis and testing of proprietary processes. It is well known that some public officials feel that the legal requirements for competitive bidding also extend to engineering contracts; and, despite the professional nature of engineering services, engineering contracts are sometimes awarded to the lowest bidder. In order to ascertain whether any such limitation exists for the engagement of an engineer by a municipality, a questionnaire was sent by the Committee to state sanitary engineers.

It appears from the results of the survey (see Table 1) that, to a considerable extent, engineers are free to fix their fees high enough to permit them to make an adequate study of processes; that is, there appear to be few legal restrictions. There is nearly always an economic restriction, however. Many public

officials feel constrained, as a matter of good business, to receive proposals for engineering services from several engineers before making a selection. In some cases the proposals are on the same specifications, and in others each engineer outlines his own specifications. In any event, where more than one

TABLE 1.—QUESTIONNAIRE, AND REPORT ON QUESTIONNAIRE, SENT TO PUBLIC OFFICIALS

THE QUESTIONS

1. Are consulting engineers required, by state law, to bid for jobs?
2. Are consulting engineers required, by any city ordinance, to bid for jobs?
3. Do all consulting engineers sell their services on open competitive bidding?
4. Do some consulting engineers compete for jobs by open competitive bidding?
5. Are all consulting engineers licensed in your state?

Item	States	Answers to Questions:				
		1	2	3	4	5
1	Washington	No	No	No	Yes	Yes
2	Arizona	No	No	No	No	Yes
3	Indiana	No	No	No	Yes[a]	Yes
4	Virginia	No	No	....	Yes	Yes
5	Kansas	No	....[b]	No	Yes	Yes
6	Massachusetts	No	No	No	No	Yes[c]
7	Michigan	No	....[d]	No	Yes	No[e]
8	Vermont	No	No	No	No	Yes
9	Arkansas	Yes[f]	No	No	No	Yes
10	Delaware	No	No	No	....[d]	Yes
11	Wyoming	No	No	No	No	Yes
12	Maine	No	No	....[b]	....[b]	Yes
13	Utah	No	No	No	No	Yes
14	North Dakota	No	No	No	No	Yes
15	Oklahoma	No	No[g]	No	Yes	Yes
16	Colorado	No	No	No	Yes	Yes
17	Mississippi	No	No	No	Yes	Yes
18	New York	No	No	No	No	Yes
19	New Hampshire	No	No	No	No	No
20	Louisiana	No	No	No	....	No
21	Rhode Island	No	No	No	No	Yes
22	New Jersey	No	....	No	Yes	Yes
23	South Carolina	No	No[g]	No	No	Yes
24	Kentucky	No	No	No	No[g]	Yes
25	Iowa	No	No	No	Yes	Yes
26	Tennessee	No	?	No	Yes	Yes
27	Wisconsin	No	No	No	Yes[h]	Yes
28	Alabama	No	No	No	No	Yes
29	Connecticut	No	....[i]	No	Yes	Yes
30	South Dakota	No	No	No	No	Yes
31	California	No[g]	....[j]	No[k]	Yes	Yes
32	Idaho	No	No	No	No	Yes
33	West Virginia	No	No	No	Yes	Yes
34	Maryland	No	No	No	Yes	Yes
35	Ohio	No	Yes[l]	No	Yes	Yes
36	Texas	No	No	No	No	Yes
37	Nebraska	No	No	No	No	Yes
38	Montana	No	No[g]	No	Yes[m]	No
39	Illinois	No	....[n]	No	....[b]	No[o]
40	Oregon	No	No	....	....	Yes
41	New Mexico	No[p]	No	No	No	Yes
42	Pennsylvania	No	....[b]	No	Yes	Yes
43	Florida	No	No	No	No	Yes
44	Georgia	No	No[g]	No	No	Yes
45	Minnesota	No	Yes[q]	No	Yes	Yes[r]
46	Missouri	....	....	....	....	....
47	Nevada	....	....	....	....	....
48	North Carolina	No	No	No	No	Yes

FOOTNOTES

[a] According to reports "yes."
[b] Do not know.
[c] Voluntary.
[d] Possibly.
[e] Not as such.
[f] If the job exceeds $5,000.
[g] "Not to my knowledge."
[h] In a few cases.
[i] Varies according to city charters.
[j] "Not certain of this."
[k] A few do not.
[l] Occasionally, by specific resolution.
[m] Probably.
[n] Unable to answer.
[o] Only structural engineers.
[p] Some communities require bids if the job exceeds $50.
[q] Some city attorneys so interpret ordinances; others do not.
[r] All engineers doing public work are required, by law, to be registered

engineering proposal is received, it is likely that the fee will be a very important element in the selection of an engineer.

It would be good business, of course—and perfectly proper—for public officials to select engineers on the basis of cost if all other considerations were equal. This presupposes, however, that the public agency knows beforehand just what is required of an engineer and can write a specification for engineering services which will fulfil all its needs (in other words, it can diagnose its own ills) and also that all engineers who tender proposals have equal ability. Neither condition is actually met in practice.

One of the greatest services that an engineer can perform is to make a thorough preliminary study and report on what is required for a client. Such a diagnosis is pure engineering of the highest type and is more costly the better the engineer; but in the end, if done well, it will result in best services at lowest cost to the client. It is the responsibility of the individual engineer to sell these principles to a prospective client, and once having sold them to retain confidence by maintaining high professional standards in the remainder of the work.

In the matter of water treatment and sewage treatment, a study of the water or waste to be treated and a study of the methods of treatment available which will produce the desired result economically would seem to be essential preliminaries to the selection of a process and the design of a plant. The obvious method for making such studies is by laboratory and pilot-plant tests. Consulting engineers serving municipalities have in general been backward in furnishing this type of service. Proprietary interests have not been backward, and their success in dealing directly with public agencies in regard to this type of service is evidence that public officials are alive to the need of it.

VII. Conclusions

It is the opinion of the Committee that most of the difficulties that have been brought to its attention are by-products of the phenomenal growth, in the United States, of industrial research. The development and promotional methods used by industry are now being brought to bear in some force upon municipal water treatment and sewage treatment. Many engineers practicing in these fields are not prepared to cope with these methods because they are unsympathetic and unfamiliar with the methods of patent protection. Some of the less scrupulous promoters have taken advantage of this situation with the result that some treatment plants have been sold to the public which either do not function properly or cost too much.

The Committee has no recommendations to make which would involve action by the Sanitary Engineering Division, or by the Society. It feels that it has fulfilled its purpose by outlining the available facts for the information of the members of the Society and by presenting its opinions for the guidance of the members.

It is the opinion of the Committee that a study of the relative effectiveness of different treatment processes is not a proper assignment for the Society. Such a study has value only if applied to a specific project, and it is the duty of the engineer on each such project to make this study. It is also the opinion of the Committee that a study of the validity of patents and patent claims is an improper task for the Society. There is no point in advising as to the validity of a patent or patent claim unless the process or equipment covered thereby is under consideration for a specific project. In this case, it is the duty of the engineer, implemented with suitable legal assistance if required, to advise the client.

The Committee believes that the best and most economical selection and design of a treatment process and plant can be obtained if the client engages an engineer for that purpose, provided, however, that the engineer is a competent expert on processes and makes thorough studies of the available

processes suitable for the project. The Committee further believes that most reliable proprietary interests would prefer to deal with competent engineers rather than directly with clients in order to insure a fair test of their processes by impartial experts. In the long run their success depends upon the existence of satisfied clients.

In order for a sanitary engineer to be a competent expert in the selection and design of treatment processes, it is essential that he should have some training and experience in process operation and development, and it is desirable that he be somewhat familiar with patents and the methods of patent protection. He must be prepared to advise his client with reference to procedure in cases of patented processes and articles so that his client may have early use of new developments and also be protected from infringement liability. The selection of a process or article should be based on its relative merits and cost, including the cost of patent royalties if any.

If an engineer is engaged to advise on methods of treatment or type of equipment and to design a treatment plant, the responsibility for the economy and successful performance of the plant rests upon the engineer. It is unethical, therefore (and it is unwise), for the engineer to use, as his own, plans and specifications prepared by the promoter or manufacturer. Of course, he must lean upon the manufacturer for guidance in the preparation of plans and specifications; and he must accept shop drawings, designs, and specifications for certain mechanical and other specialized equipment. He is obligated, however, to check the designs and claims of the manufacturer and to assure himself that the manufacturer's process or equipment will function as claimed. If necessary, specialized consultants must be engaged by the engineer to assist in this procedure.

Since the responsibility for successful and economical performance of a treatment plant rests with the engineer, he should assume the full responsibility for the design, selection, and testing of processes. He may properly enlist the cooperation of the United States Public Health Service, state departments of public health, or other suitable agencies in his studies and tests of treatment methods; but the responsibility for the results remains with the engineer and cannot be shifted to others without impairment of his professional standing as a process expert.

The responsibility for the development of the art of water treatment and sewage treatment rests squarely with the engineering profession. The public looks to the professional engineer as the legitimate expert in this field. If he is to maintain this position, he must support research and development actively, and he must be prepared to examine and test new developments, with competence, to the end that the public may procure the early use of worthwhile improvements in the art.

Respectfully submitted,

Don E. Bloodgood — F. M. Veatch
Wellington Donaldson — Frederick H. Waring
Thomas R. Camp, *Chairman*
Committee on Evaluation of Professional Objectives in the Design of Sanitary Engineering Works

January 16, 1946

APPENDIX

First Report of Committee on Evaluation of Professional Objectives in the Design of Sanitary Engineering Works; January, 1945

[For this printing, the first two paragraphs of the original First Progress Report are used in Section I of this report.—Ed.]

The request for the formation of the Committee appears to have arisen mainly as a result of the growing practice by promoters of patented processes or equipment of controlling or limiting, by the use of patents, the selection of treatment processes for water and sewage. There appears also to be a growing practice, by promoters, of selling direct to clients with engineering services furnished free, thus by-passing engineers in private practice.

The Committee has had one all-day meeting and has corresponded freely during the past year. One of its first tasks was to agree upon the scope of its activities and to attempt to set definite objectives. The Committee believes that it has made progress in defining its own objectives; but, due to the highly controversial nature of the matters to be studied and to the pressure of war duties, it has not been possible for the Committee to submit a final report at this time.

The assignment to the Committee makes no distinction between water treatment and sewage treatment works for public agencies and such works for private industry. Although the Committee believes that its appointment arose primarily from practice trends in public works, the Committee nevertheless feels that its studies and recommendations should include private and industrial sanitary engineering works. An important distinction must be drawn between these two classes of works because of the fact that competitive bidding is required by law on public works and not so required on private works.

The absence of legal restrictions on the award of contracts for private sanitary engineering works has favored a rapid growth in the use of patented processes and equipment. In seeking new outlets for their business, it is quite natural that promoters should extend, to public works, business practices which have been successful in private works.

There are three interests involved in the matters to be studied by the Committee—namely, the client, the professional engineer, and the promoter of the proprietary processes and equipment. The Committee is a creature of the Society and this report must obviously be directed to the members of the Society. Although it is hoped that the report may influence the actions of clients and promoters it can only do so if it makes recommendations which are advantageous to those interests.

The Committee expects to continue its deliberations during the year with the hope that a constructive report can be submitted at the next Annual Meeting. In the meantime the Committee solicits assistance from all who are interested in this problem.

AMERICAN SOCIETY OF CIVIL ENGINEERS

Founded November 5, 1852

DISCUSSIONS

FUTURE OF LAKE MEAD AND ELEPHANT BUTTE RESERVOIR

Discussion

BY HUGH STEVENS BELL, E. W. LANE, AND F. E. BONNER

HUGH STEVENS BELL,[92] ESQ.[92a]—This generation and the next almost certainly will develop every desirable large reservoir site west of the Mississippi River. In effect, this generation is in the midst of reaping a priceless harvest. The crop is not like grain or timber which can be grown again, but like gold or petroleum; and, as Carl Sauer of the University of California, at Berkeley, has stated, when the supply of a particular mineral is exhausted, the harvest of all geologic time has been reaped.[93] Among American engineers, Mr. Stevens is apparently the most thoroughly aware of the fact that this philosophy applies also to reservoir sites and that it places definite obligations upon the generations that harvest them.

Under the heading, "Space Occupied by Deposits," Mr. Stevens writes,

> "For the Lake Mead deposits no better estimate of the specific weight can be made than to use the composite value of γ = 65 lb found for Elephant Butte Reservoir."

By using this value to calculate the life of Elephant Butte, the author apparently makes the assumption that these sediments will not increase in density with time. By using it to estimate the life of Lake Mead he implies, at least, that it is the future value; and, judging by the assumptions made for Elephant Butte, the present value also.

How much space will be occupied by a given tonnage of sediment is, perhaps, the most perplexing question that must be faced in estimating the life of any reservoir. Consequently, it is unfortunate that no extensive sediment surveys, like those for Elephant Butte Reservoir, have been made at Lake Mead How-

NOTE.—This paper by J. C. Stevens was published in May, 1945, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: October, 1945, by D. C. Bondurant, John H. Bliss, Luna B. Leopold, Carl B. Brown, and G. E. P. Smith; and November, 1945, by Albert E. Coldwell, Walter B. Langbein, C. S. Howard, Charles Kirby Fox, H. V. Peterson, L. C. Crawford and P. C. Benedict, Charles P. Berkey, and Stafford C. Happ; and December, 1945, by Berard J. Witzig.

[92] Soil Conservationist, Cooperative Hydraulics Laboratory, California Inst. of Technology, Pasadena, Calif.

[92a] Received by the Secretary October 26, 1945.

[93] "The Relation of Man to Nature in the Southwest: A Conference," *The Huntington Library Quarterly*, Vol. VIII, No. 2, February, 1945, p. 123.

ever, the Bureau of Reclamation and the U. S. Geological Survey have collected many pertinent data and there are certain others now available which, although they are not adequate for determining definitely the specific weight of sediment deposits at Lake Mead, furnish a basis for comparing these deposits with those at Elephant Butte. After studying these data the writer has come to the conclusions that: (1) At present, sediment deposits have a considerably lower specific weight at Lake Mead than at Elephant Butte, and (2) the appropriate value of γ for use in estimating the probable life of Lake Mead may be less than 65 lb, the value used by the author. A somewhat detailed analysis of the volume-weight problem and a discussion of sedimentation at Lake Mead during the ten years ending in 1945 are presented in the following paragraphs, accompanied by data and the writer's interpretation of them.

The Volume-Weight Problem.—There are several reasons why the present sediment deposits in Lake Mead should have a lower mean specific weight than do those at Elephant Butte. The manner in which a reservoir is operated, the conditions under which sediments are laid down, the size of the particles, the age and thickness of the deposits, and other variables, such as the degree of flocculation, influence the rate of compaction and, consequently, the unit weight the deposits will have attained at any particular time. Where obvious differences in these factors are found, they almost invariably indicate conditions favoring greater specific weight at Elephant Butte.

Reservoir Operation.—The extent to which a reservoir is drawn down during normal operation is an important factor in determining the specific weight of deposits. Regarding Elephant Butte, the late Henry M. Eakin wrote,

> "During low-water stages in the average year most of the reservoir basin above the Narrows is uncovered. Sediment deposits dropped here during floods are exposed to air and hardened."[94]

Approximately 40% of the basin lies above the Narrows, so it is probable that at least half of the reservoir bottom is exposed sometime during the average year. Frequently the reservoir is filled to less than 50% of capacity; occasionally it is filled to less than 25%.

The situation at Lake Mead is very different. Since June, 1938, there has never been a time when less than 73% of the capacity to spillway-crest elevation was occupied, or when less than 80% of the basin was submerged. As will be shown in discussing the conditions of sedimentation, this is of greater importance than the ratio of the exposed areas indicates.

Clay and colloidal particles, especially if they are flocculated, form deposits having very low specific weights. When sediments of this kind are brought to the dam by density currents, as they are at both reservoirs, they sometimes are discharged at Elephant Butte but they always are stored at Lake Mead. If the 6.4 million tons of fine material that passed through the outlet works during the period of record at Elephant Butte had been stored, they would have occupied enough space, at 35 lb per cu ft, to have reduced the specific weight calculated by the author from 65.3 lb to 64 lb—a change of 2%.

[94] "Silting of Reservoirs," by Henry M. Eakin, revised by Carl B. Brown, *Technical Bulletin No. 524*, U. S. D. A., Washington, D. C., 1939, p. 94.

Conditions of Sedimentation.—The Rio Grande, upon entering the reservoir, often spreads out over broad areas covered with willow, tamarisk (salt cedar), and cottonwood. This encourages the deposition of even the finest particles at levels normally exposed to the atmosphere. Mr. Eakin states that the upper part of the basin is covered with above-crest deposits and that the surface sediment is characteristically very fine textured, containing a high percentage of true clay and colloidal matter.[94] Although drying makes little or no difference in the specific weight of deposits of sand, it makes important changes in that of the finer sediments. From data available to them, E. W. Lane, M. Am. Soc. C. E., and Victor A. Koelzer, Jun. Am. Soc. C. E., have estimated that fine sediment, deposited under conditions where it has ample opportunity to dry out, will reach the same specific weight in a short time that it would under less favorable conditions in a 1,000-yr period.[95] At Elephant Butte, fine deposits in the upper basin have ample opportunity to be compacted by drying and, although they may be wet at a later date, and may expand somewhat, they will not reach their former bulk. Furthermore, there is little likelihood that they will be reworked and redeposited at some later date in areas where they will remain permanently submerged and, consequently, have a comparatively low specific weight at any given time.

At Lake Mead, however, the situation now, and for many years to come, is entirely different. The upper 40 miles or so of the basin lie in Lower Granite Gorge down which (see Fig. 14) the Colorado River advanced its delta approxi-

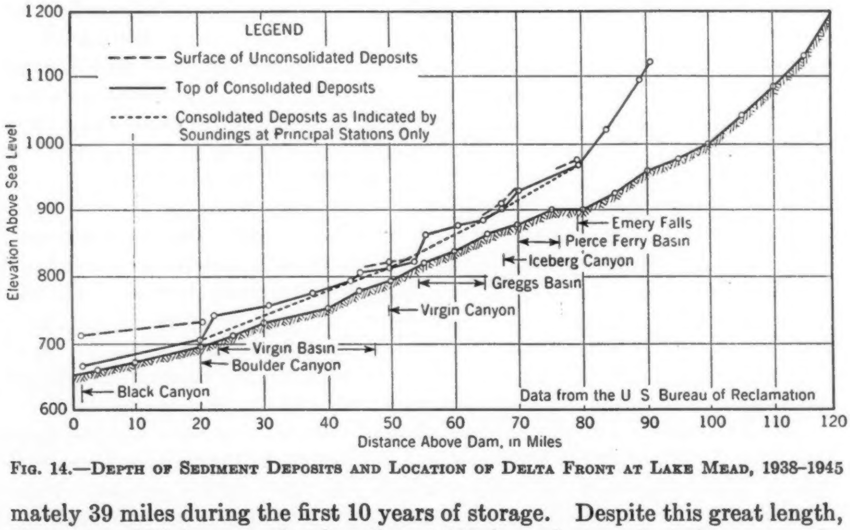

FIG. 14.—DEPTH OF SEDIMENT DEPOSITS AND LOCATION OF DELTA FRONT AT LAKE MEAD, 1938–1945

mately 39 miles during the first 10 years of storage. Despite this great length, the mean width is somewhat less than 1,000 ft—a feature that encourages the rapid reworking of deltaic deposits and the washing out of a high percentage of the fine particles which are subsequently deposited in areas that are permanently submerged.

[95] "Density of Sediments Deposited in Reservoirs," by E. W. Lane and Victor A. Koelzer, *Report No. 9* of "A Study of Methods Used in Measurement and Analysis of Sediment Loads in Streams," U. S. Engr. Sub-Office, Iowa Inst. of Hydr. Research, Iowa City, Iowa, 1943, p. 51.

In 1941 Thomas C. Mead of the Bureau of Reclamation sent the writer a sediment sample taken 2 ft above water level from the bank of a channel the river had cut through delta deposits about 100 miles above Boulder Dam. An analysis, made after the sediment had been dispersed with sodium oxalate, showed 60% sand ($> 50\ \mu$), 39% silt ($< 50 > 5\ \mu$), and 1% clay and colloidal material ($< 5\ \mu$). Upon drying out, a sediment of this composition experiences an 8% to 10% increase in specific weight, but delta deposits at Lake Mead tend to remain moist because, although the delta is perhaps 40 miles long, its area is only from 6 sq miles to 8 sq miles, including the channel of the river.

At Lake Mead, then, any increase in the specific weight of sediment deposits resulting from exposure to the atmosphere may be considered to be almost negligible for many years to come, whereas at Elephant Butte drying has been and, apparently always will be, an important factor.

Particle Size.—Analyses of more than 600 samples from each stream, taken under comparable circumstances, indicate a strong similarity in the sediment-size composition. The Rio Grande samples contained 22% sand, 37% silt, and 41% clay; and samples from the Colorado River, 28% sand, 69.2% silt, and 2.8% clay. Because the former were deflocculated before analysis and the latter were not, the data for silt and clay content are not comparable, but there are good reasons for assuming that actual differences are small. However, the samples are weighted neither for sediment concentration nor for stream discharge and, consequently, do not give a true picture of the actual composition of the total sediment load. Unpublished computations by Carl B. Brown, Assoc. M. Am. Soc. C. E., show that proper weighting of the Colorado River samples indicates the actual composition was 49.4% sand, 13.3% coarse silt, and 37.3% fine silt, clay, and colloids. This corresponds very closely with unpublished estimates by the U. S. Geological Survey which show that the suspended load passing the Grand Canyon gaging station in 1935 was composed of 45% sand, 15.2% coarse silt, and 39.8% finer material.

Mean Age of Deposits.—In 1945 the mean age of deposits in Elephant Butte Reservoir was about three times that of those in Lake Mead. Apparently time makes little or no difference in the specific weight of deposits of sand and coarser sediments, but becomes increasingly important as the size of the particles diminishes. On the basis of time alone, then, the deposits in Lake Mead should now be less dense than those at Elephant Butte; but, since the life of each reservoir, under present conditions, is estimated to be approximately a century and a half, the eventual difference from this cause is negligible.

The Thickness of Deposits.—Lake Mead is more than two and one-half times as deep as Elephant Butte Reservoir and, consequently, the possible increase in density due to the weight of the overlying sediments should be greater. It has been assumed that the sediments entering the two reservoirs are similar, but it has been shown that the venting of clay and colloidal material transported by density currents at Elephant Butte is a factor that tends to increase the specific weight of deposits there by about 2%.

Since the fine particles are highly flocculated at both reservoirs, it might seem that no difference in specific weight can be attributed to this factor. There is some evidence, however, that flocculation materially decreases the rate

of compaction of submerged deposits. If so, it would tend to decrease the mean specific weight at Lake Mead more than at Elephant Butte because practically all fine sediment is retained and remains submerged in the former, but not at the latter.

Summarizing, a comparison of the factors involved indicates that the sediments deposited at Elephant Butte Reservoir should now have a mean specific weight greater than do those at Lake Mead. Eventually this difference will be reduced to an unknown degree by the mere passage of time and, perhaps, to a greater extent by the accumulation of much thicker deposits at Lake Mead. Although a value of $\gamma = 65$ lb per cu ft is now probably too high for Lake Mead, it may be approximately correct for the year 2075 and, therefore, is an acceptable value for use in calculating the life of the reservoir.

Nature and Extent of Sediment Deposits in Lake Mead.—Data on the depth of deposited sediment and on particle size have been gathered for several years at a number of stations in Lake Mead by the Bureau of Reclamation. Fig. 14 shows the depth of sediment deposited at these stations as of February, 1945, ten years after storage began. Fig. 15 is a map of the reservoir and shows the

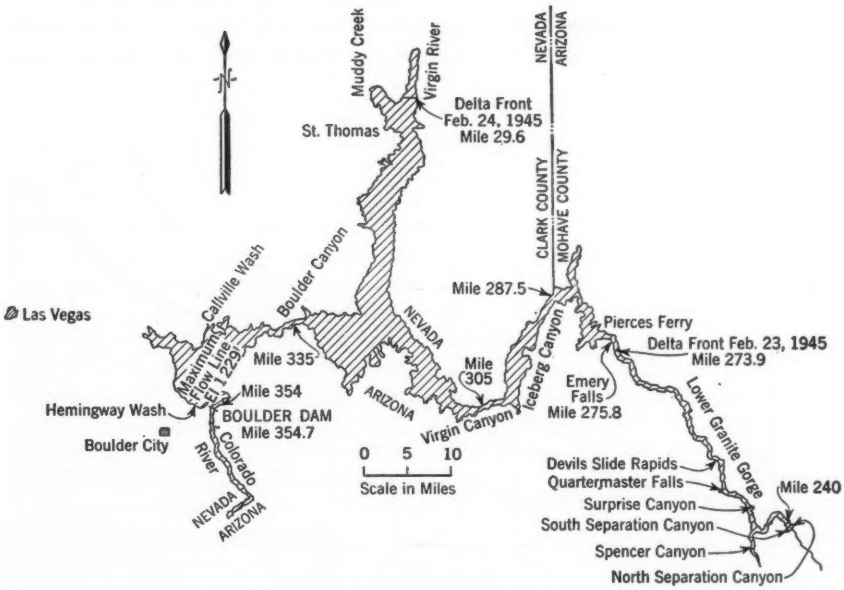

FIG. 15.—MAP OF LAKE MEAD SHOWING LOCATION OF PRINCIPAL SAMPLING STATIONS AND POSITION OF DELTA FRONTS ON THE COLORADO AND VIRGIN RIVERS IN FEBRUARY, 1945

location of the principal sampling stations: Emery Falls, Iceberg Canyon, Virgin Canyon, Boulder Canyon, and near Boulder Dam. All stations are located at narrow places. Because the velocity of density currents tends to be somewhat higher at such locations, the quantity of deposition there is less than elsewhere, as is shown by Fig. 16.

By using these data, the contour maps of Lake Mead prepared by the Soil

Conservation Service, the author's estimate of the incoming sediment load, and various bits of pertinent data from the U. S. Geological Survey, a rather general picture can be constructed of sedimentation during the ten years ending in 1945. It also becomes possible to make rough estimates of the probable mean specific weight as of 1945.

Volume of Delta Deposits.—In February, 1945, the delta was approximately 40 miles long and had a cross-sectional area of slightly more than 5 acres at its lower end. A conservative estimate of the volume of the delta can be computed on the basis of a wedge as wide as the river at the upper end of the delta and with a base of 2.3 acres, and two pyramids each having a basal area of 1.35 acres—all with heights of 40 miles. These have a combined volume of 432,000 acre-ft. Irregularities in Lower Granite Gorge, and deposits laid down during high reservoir stages, could account for an additional 20%. The author has stated that the opportunity for extensive deposits above lake levels is lacking when the river joins the reservoir in a narrow canyon. Perhaps it should be emphasized that, although this is true at Lake Mead now, the advance of the delta into the various basins, and particularly into Virgin Basin, will afford a future opportunity which now is lacking. Sediment storage of this kind probably deserves consideration in any attempt to predict the life of Lake Mead.

The writer is of the opinion that the delta contained at least 500,000 acre-ft

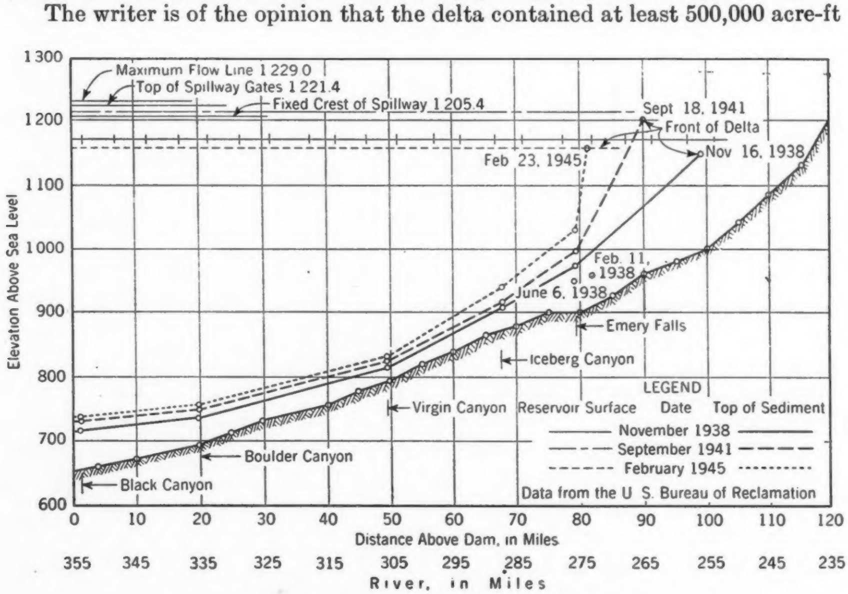

FIG. 16.—SEDIMENT DEPOSITS, LAKE MEAD, IN MAY, 1940

in February, 1945. Although storage had been in progress for 10 years, the delta represented the accumulation of sediment during only 8.75 years. In February, 1945, the toe of the delta had reached a point at which the original bottom elevation was 900. On June 26, 1935, less than 5 months after storage began, the reservoir surface had reached that elevation and was at El. 905.2 as

late as April 14, 1936. Never since June 11, 1936, has it been below El. 1000. During the first 15 months of storage, therefore, sediment of the kind which subsequently formed the delta was being laid down as bottom deposits downstream from the point reached by the toe in February, 1945. On the basis of the estimated volume of the delta, this sediment should occupy approximately 70,000 acre-ft.

Mr. Stevens, in Table 8, estimates the average annual accumulation of sediment in Lake Mead for the period from February 1, 1935, to September 30, 1942, to be 252 million tons. Of this total, according to Table 7, the Virgin and Muddy rivers contribute 15.4 million tons. The contribution of other streams entering Lake Mead below the delta may be expected to increase this quantity to 25 million tons. Their deltas, and delta-like deposits laid down while Lake Mead was filling, are assumed to have a volume of 65,000 acre-ft, making a grand total of 635,000 acre-ft for such deposits during the first 10 years of storage.

Specific Weight of Delta Deposits.—From numerous specific-weight determinations, the late Samuel Fortier and Harry F. Blaney,[44] Members, Am. Soc. C. E., derived an average value of 84.5 lb per cu ft for Colorado River deposits near Yuma and Laguna Dam. Subsequently the U. S. Geological Survey made determinations (reported by Messrs. Lane and Koelzer) on 105 samples of Colorado River channel and flood-plain deposits from Lees Ferry to Yuma. These have an average specific weight of 87.5 lb per cu ft. Of the 105 samples, fifty six were taken between Grand Canyon and Parker Dam and of these forty three seem most likely to represent delta deposits under present conditions. Their average specific weight is 89.4 lb. All had been exposed to the atmosphere; but, since they contained an average of 73% sand, 24% silt, and 3% clay, and few were in locations that would have permitted thorough drying, their specific weight could have been increased only slightly by exposure. Any increase from this source might be more than offset in the delta deposits by compaction attributable to time and the weight of overlying sediment. The writer estimates the mean specific weight of delta deposits at approximately 90 lb per cu ft. At this value the estimated volume, 635,000 acre-ft, would weigh nearly 1.25 billion tons, which is slightly less than 50% of the 2.52 billion tons of sediment of all sizes which are estimated to have reached the reservoir during the first 10 years of storage.

Volume of Density-Current Deposits.—Sediment carried beyond the delta is transported by density currents and must be composed very largely of silt and clay because the velocities are too low, ordinarily, to keep sand particles in suspension. Before reaching Boulder Dam, these currents must pass through a series of basins in which they have an opportunity to spread out and greatly decrease their velocity. It would be surprising if much sand is carried beyond Pierce Ferry Basin. Apparently most of the silt is deposited in Greggs and Virgin basins (Fig. 15), because samples taken since 1942 at Boulder Canyon and Black Canyon indicate that less than 3% of the sediment in the lower 20 miles of the lake is coarser than 15 μ.

[44] "Silt in the Colorado River and Its Relation to Irrigation," by Samuel Fortier and Harry F. Blaney, *Technical Bulletin No. 67*, U. S. D. A., Washington, D. C., 1928, p. 4.

Estimating the volume of the deposits beyond the delta is made extremely difficult because the sampling stations are located in canyon sections where velocities are comparatively high and the opportunities for deposition correspondingly low. Almost no data are available for basin areas where deposits are broadest and deepest. Soundings were made in the basins during May, 1940, however, and the data have been used in Fig. 16 which illustrates nicely the difference in thickness of deposits at narrow and at wide sections. The basins actually contained much more sediment than was indicated by data from the regular stations.

The Soil Conservation Service has prepared topographic maps of the entire Lake Mead area on a scale of 1 : 12,000. By using these maps with data from the regular stations the writer calculated that, in February, 1945, deposits below the delta had a volume of 860,000 acre-ft. Because the data are from canyon sections only, the calculated volume is probably at least 50% too low; it should be increased to 1,290,000 acre-ft, of which 70,000 acre-ft are sandy bottom deposits already accounted for in discussing the delta.

Specific Weight of Density-Current Deposits.—Of the sediment that is assumed to have reached the reservoir during the first 10 years of storage, slightly more than 50%, or 1.27 billion tons, apparently has been transported by density currents. If this occupies 1,290,000 acre-ft, as estimated in the preceding paragraph, it has a specific weight equal to 47.5 lb, and the mean for all sediment in the reservoir is 62.5 lb per cu ft.

There is considerable evidence to indicate that the mean specific weight of density-current deposits is less than 47.5 lb. Of the large number of samples taken above Boulder Canyon by the Bureau of Reclamation prior to February, 1945, only two had greater concentrations, three others showed more than 45 lb, four had between 40 lb and 45 lb, and seventy three had less than 40 lb. Of the hundreds of samples taken from Boulder Canyon to the dam, only four had concentrations greater than 30 lb and the heaviest of these showed only 32.75 lb.

Fig. 17(*a*) shows generalized curves of sediment concentration with respect to time and elevation at Boulder Dam (mile 354.7) during the period from May, 1937, through December, 1943; and Fig. 17(*b*) shows similar curves for the station at Black Canyon (mile 353.5) from January, 1944, through June, 1945. Each small circle shows the elevation and date of collection of a sample that was analyzed for sediment content; the samples marked × (Fig. 17(*a*)) in the first few months represent known silty water but analyses are lacking. The sediment concentration, expressed as percentages by weight, was determined by dividing the weight of the sediment after drying by the weight of the original sample. For convenience the concentration is shown also in pounds per cubic foot.

The curves indicate an increasing specific weight at a lessening rate with time, and follow the general pattern that would be anticipated for clay compaction curves. The apparent inconsistency of the 35% curve during 1939 and 1940 probably reflects difficulties encountered in developing a sampler and a sampling technique that could be used effectively in concentrations greater than 30%. The 40% curve is wholly extrapolated, but it is reasonable

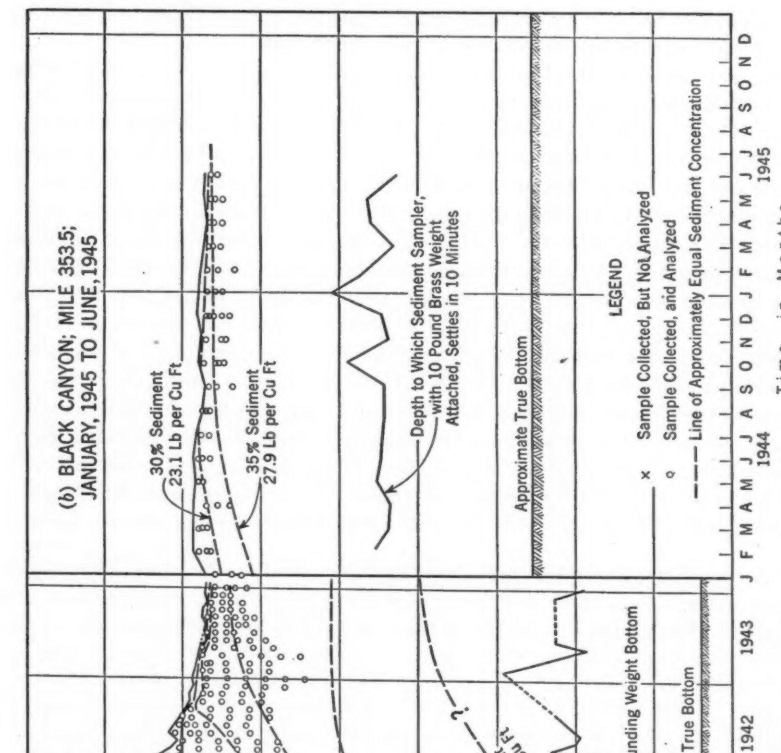

FIG. 17.—SEDIMENT CONCENTRATION IN LAKE MEAD WITH RESPECT TO TIME AND ELEVATION
(a) UPSTREAM INTAKE TOWERS; MILE 354.7; JUNE, 1937, TO DECEMBER, 1943
(b) BLACK CANYON; MILE 353.5; JANUARY, 1944, TO JUNE, 1945

to assume that such densities now exist. A specific weight of 30 lb per cu ft is approximately correct for 225,000 acre-ft of sediment deposited between Boulder Canyon and the dam prior to February, 1945.

The curve showing the top of the silty water is a succession of peaks rising sharply on the left when density currents reach the dam and subsiding during months devoid of major density flows. The curve of bottom depths is erratic, probably because of differences in the types of sounding weights used and in the time allowed for settlement. Until 1944 "bottom" was the point to which a 10-lb weight would settle in 10 min. The weight usually was brass, but a lead one was used occasionally. Since 1944 the brass weight has been attached to a sampler and the pair has been allowed to settle as usual. This accounts for the greater elevation of the "bottom" as indicated in Fig. 17(*a*).

Since 1938 samples have been taken at several depths each time the Bureau of Reclamation has investigated the sediment concentration at the various stations from Boulder Canyon to the delta. Because of the inability of the sampler to penetrate to all depths, the densest sample taken on each occasion has been assumed to be most nearly representative of the mean specific weight for all sediment deposited at the station being investigated. An average value for each station has then been determined from these selected samples, and the results are shown in Table 16. The specific weight shown for the

TABLE 16.—SPECIFIC WEIGHT γ OF DENSITY-CURRENT DEPOSITS (ESTIMATES BASED ON PUBLISHED AND UNPUBLISHED DATA FROM THE BUREAU OF RECLAMATION, U. S. DEPARTMENT OF THE INTERIOR)

Sampling station (Fig. 15)	Location (river mile)	No. of samples	Description of sediment	Average sampling depth (ft)	Specific Weight γ (Lb per Cu Ft)					
					Observed	Adjusted	Col. 6 / Col. 7	Average; adjacent stations	Between stations (%)	Col. 9 × Col. 10
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Emery Falls....	275.8	14	15% > 20 μ	4.1	35.5	59.2	0.60			
								53.6	20	10.7
Iceberg Canyon	287.5	10	10% > 20 μ	6.0	31.2	48.0	0.65			
								44.9	16	7.2
Virgin Canyon.	305.3	28	2% > 20 μ	1.8	28.9	41.2	0.70			
								35.6	37	13.2
Boulder Canyon	334.9	28	2% > 15 μ	8.7	24.6	30.0	0.82			
								30.0	27	8.1
Black Canyon..	{353.5 354.7}	....	3% > 15 μ	....	..	30.0	....			
Adjusted specific weight γ for all density-current deposits (lb per cu ft)...							....	....	....	39.2

Boulder Canyon station is approximately 82% of the 30 lb which more complete data seem to indicate is probable for sediment of the same composition near the dam; and the average value increases with distance from the dam. Col. 5, Table 16, shows the average depth the sampler penetrated the deposits; and, since these values are small compared to the total depth, they justify the assumption that the samples are somewhat less dense than the mean value for the sediment at the location. Col. 4 indicates that sediment becomes coarser

with distance from the dam; and Col. 5 shows that the firmness of the deposited sediment, as indicated by the depth the sampler penetrated, decreases as the particle size decreases. Deposits at Virgin Canyon apparently do not conform, possibly because that is the point of minimum deposition.

Because firmness and coarseness give a measure of the specific weight, the writer has adjusted the values recorded in Col. 6 by assuming that these represent a percentage, shown in Col. 8, of the probable mean specific weight for the various stations. These adjusted values, which are admittedly only the writer's best guess, are shown in Col. 7, Table 16. The average of these for each pair of adjacent stations is taken as representative of the mean specific weight of the sediment deposited between those stations, as indicated in Col. 9. From his previous estimate of the volume of density-current deposits, the writer distributes them as follows: From the delta to Iceberg Canyon, 20%; from Iceberg Canyon to Virgin Canyon, 16%; from Virgin Canyon to Boulder Canyon, 37%; from Boulder Canyon to the dam, 27%. This distribution yields 39.2 lb per cu ft as the mean specific weight for density-current deposits and 54.4 lb for all deposits in the reservoir.

Summary.—This discussion, perhaps, has shown the hopelessness of the task of finding the correct or even a convincing answer for the volume-weight problem from the data now available for Lake Mead. The writer believes that the answer, for the present, lies somewhere between 54 lb per cu ft and 58 lb per cu ft, possibly rather close to the former. He believes also that 62.5 lb per cu ft, the value suggested by Mr. Blaney in discussing this problem in 1929,[96] might well be used as the basis for calculating the life of the reservoir.

E. W. Lane,[97] M. Am. Soc. C. E.[97a]—The future of Lake Mead and Elephant Butte Reservoir is of major importance to many people; and it is therefore very desirable that, as far as it is possible to do so, the future of these reservoirs be accurately predicted. The author should be commended for bringing this important subject up for discussion, and for collating the widely scattered data on which such a study must be based.

From the standpoint of sediment deposits, the future of these reservoirs depends principally upon three factors: (1) The amount of sediment brought down by the streams supplying the reservoirs; (2) the amount of sediment that will pass out of the reservoirs; and (3) the position in which the sediment will be deposited upstream from the dam. The author has collected many data on the amount of sediment which the streams have brought down in the past, and therefore may reasonably be assumed to bring down in the future. As far as the first factor is concerned, the paper seems to be quite complete. The second factor is covered by the assumption that all the sediment brought down will be deposited above the dam and that none of it will pass through to the river below. The third factor is also covered by an assumption—that all the material brought down will be deposited in the reservoir below the maximum flow line. These latter two assumptions are those ordinarily made in studies of the life of reservoirs, but the writer believes that, if a degree of accuracy

[96] *Transactions*, Am. Soc. C. E., Vol. 94 (1930), p. 1139.

[97] Eng. Bldg., Iowa City, Iowa.

[97a] Received by the Secretary November 13, 1945.

is desired in the estimate of reservoir life comparable with that obtained in the estimated quantity of sediment inflow, a detailed examination of these two assumptions is necessary to be sure that errors of considerable magnitude are not introduced. The importance of an accurate estimate should justify such an examination, and the writer's discussion will be confined largely to this subject.

The assumption that all the sediment is deposited in the reservoirs is correct for the present conditions, but will become less so as the reservoirs are filled with sediment. As soon as Lake Mead is filled up to the bottom of the gate towers, sediment brought down by the underflows may begin flowing out through the turbines. In fact, this may occur even before the filling reaches this height, since the momentum of the movement of the underflows may cause the underflows to rise considerably upon reaching the dam, because the force of gravity acting on them is so nearly balanced by that acting on the surrounding clear water. As the reservoirs fill, the slope of the bottom will be increased, since the level at the outlet end will remain stationary and the length of the reservoir will be reduced. The velocity of the underflows will therefore be increased and more fine sediment will be transported to and through the dam. Since the underflows in Lake Mead under present conditions are known to carry very large quantities of sediment, the sediment outflow from this reservoir may eventually become a considerable part of the inflowing sediment.

The discharge of the fine sediment carried by the underflows tends to decrease the filling rate not only because a certain weight of sediment is eliminated but also because the unit weight of the part deposited in the reservoir is greater than the average. Mr. Stevens proposes a value of 65 lb per cu ft. Assuming that this value is correct for the average density of all the inflowing material, if considerable quantities of the fine sediment pass through the dam, the unit weight of the remainder will be somewhat greater than the average (since the unit weight of the fine material is considerably less than the average); and the space that the sediment will occupy will therefore be less than the use of the 65-lb weight would indicate.

The assumption covering the third factor—that the sediment will all be deposited below the level of the maximum flow line—is equivalent to the assumption that the sediment in the water will move as readily as the water alone. This is not the case, and such an assumption may introduce errors of considerable magnitude. The fact that the sediment upstream from a dam is not deposited on a level surface like the water surface of a reservoir, but is deposited on a slope, has been stated by the author. In the case of Bear Creek in California, the deposits above the level of the dam were of the order of 10,000% of the water storage capacity of the basin. Since, above water level, storages of such great magnitude can occur, it appears unsafe to dismiss this factor with a mere assumption. The author states (see heading, "Part I. Elephant Butte Reservoir: Probable Useful Life"):

> "Deposits in the valley above the [Elephant Butte] reservoir have been enormous so that a substantial portion of the water-borne sediments is arrested before it reaches the reservoir."

In the future, as the deposits in Elephant Butte Reservoir extend farther into the reservoir, the proportion of sediment that will be deposited above the water level will increase. This deposit not only will occur within the original area of the reservoir, but will eventually extend many miles upstream beyond this area. The valley above the reservoir is wide and is thickly covered with willows and brush. At times the river breaks into this densely covered area and deposits large volumes of sediment. Although tamarisk growth does not appear to have reached the density of that above the McMillan Reservoir, the growth is reported to be increasing rapidly along the Rio Grande, and before long may considerably retard the sediment inflow into the reservoir. The construction of flood control reservoirs upstream may reduce the sediment inflow further. For these reasons, the writer believes that (assuming that the years of record indicate an accurate average sediment flow) the straight line in Fig. 3, giving the percentage of the capacity of the reservoir occupied by sediment at various times is not justified; that the curve would probably be convex upward, as the portion based on actual records indicates; and that the life of the reservoir would be considerably longer than predicted by the author.

The writer is also unable to agree with the author's statement that, because the river joins the reservoir in a narrow canyon, the upper end of Lake Mead would not aggrade above the reservoir level. When the reservoir is filled, the river will flow the entire length of the reservoir in a channel through the sediment and will have a cross section of the same order of magnitude as it had before the reservoir was constructed. If the deposits are no higher at the upper end of the reservoir than at the dam, the river will have to flow the entire 115-mile length of the reservoir with no fall. Obviously, this will not occur. At a number of places along its course below the Grand Canyon, the Colorado River flows through canyons, similar to those at the upper end of Lake Mead, on a slope which does not differ greatly from the slope where the valley is wider. In times of flood, the bottom of the river may scour deeply but it refills as the flood recedes. When the reservoir is nearly filled with sediment, the slope of the deposits through the canyons at the head of Lake Mead should not differ greatly from that in the canyons below Lake Mead. Because the valleys in the upper end of the basin are narrow, the volume of the sediment deposits above reservoir level in this region will not be relatively as great as in the case of the Elephant Butte Reservoir; but, before the reservoir is filled, the sediment level in the upper end of the reservoir will probably be materially above the high-water level. In any event, there are large areas in Lake Mead, at considerable distances upstream from the dam, where the reservoir is wide and sediment will be deposited to a considerable height above the maximum water level before the reservoir is filled with sediment.

Some deposit of sediment above maximum water level in the reservoir is practically certain to occur eventually in any reservoir into which sediment-laden water flows. Whether or not deposits will build up rapidly enough to cause trouble in a sufficiently short time to be important depends upon the amount and size of the sediment brought in, the volume of storage space available, the condition of confinement of the stream, the fluctuation of water levels in the reservoir, and possibly other factors. Each reservoir is a special case and must be studied in detail to make certain what the result will be.

When such special studies were made, it may even be found that the effectiveness of the reservoir may be destroyed by less material than is necessary to fill the reservoir to the maximum water level, and this condition can result from the same cause as that which produces deposits above the maximum water level—namely, sediment will not move as readily as water. This action will take place to a certain extent in the Virgin-Muddy Creek branch of Lake Mead. The data given by the author indicate that about 7% of the total sediment coming into the reservoir is brought in by the Virgin-Muddy branch and rough computations indicate that about 20% of the capacity of the reservoir is in the Virgin-Muddy branch. Since comparatively little sediment brought down by the Colorado River will flow up into the Virgin-Muddy branch of the reservoir, the Colorado River branch and the reservoir below the junction of the two branches might be filled with Colorado River sediment before the Virgin-Muddy branch is filled. When this occurs, the useful life of the reservoir, from a storage standpoint, will end, since the Virgin-Muddy branch will become a lake, the surface of which cannot be fluctuated by varying the water level at the dam. Therefore, such a lake would have no useful storage. Rough computations indicate that in dry years the flow of the Virgin River may not even supply the evaporation on this lake and that if this occurred the lake would sometimes be entirely cut off from the Colorado River.

Without a detailed study, it is not possible to determine whether the magnitude of the deposits above the high-water level plus the discharge of sediment in the underflow passing through the turbines, will be greater than the space which will not be filled by sediment in the Virgin-Muddy branch. However, sufficient data are available to make a detailed study to determine with reasonable accuracy, not only what the useful life of Lake Mead will be under present conditions, but also what the conditions will be in the reservoir at any future date. The technique for such studies has already been largely developed in connection with investigations of other reservoirs. To explain it in detail would exceed the reasonable scope of discussion. Briefly, it consists of dividing the future record into periods and estimating the volume of the deposits in the various parts of the reservoir for the first period from available data on actual deposits or from the size composition of the sediments. A similar estimate is then made for the second period for the altered conditions of the reservoir brought about by the deposits in the first period. The process involves considerable computation; but, in view of the importance of the conclusions, it should be justified.

The fact that reservoir deposits tend to consolidate with time introduces an uncertain factor in reservoir life determinations. The density value of 65 lb per cu ft observed in the Elephant Butte Reservoir is for sediment with an average consolidation period of about 13 years. If the life of the reservoir is 158 years, as estimated by the author, the deposits would have an average consolidation period of 79 years and should, therefore, be somewhat more dense. Data for estimating this rate of consolidation are not readily available, but considerable new knowledge could no doubt be obtained by the use of the consolidation analysis methods developed in soil mechanics.

F. E. Bonner,[98] M. Am. Soc. C. E.[98a]—Another notable contribution to the subject of reservoir siltation in the Southwest is made by this paper. The comprehensive estimate of the silt yield of Colorado Basin from recently collected data is particularly interesting. Although the related forecast of storage depletion is less optimistic than earlier estimates, the difference is not enough to justify widespread alarm. Since ample space has been provided in the past for the writer to expound his views on this subject,[99,100] the present discussion is confined to only one phase which deserves more consideration.

As alluvial debris accumulates in Lake Mead, the space so occupied is not wholly lost for water storage. Even in that dim, distant day when the lake may be almost filled with gravel, sand, and silt, it will not have ceased to exist as a storage reservoir. Instead, it will have been transformed into a vast ground-water storage reservoir. True enough, the capacity will be reduced, but it will still be substantial. According to the author's estimate, the debris will be consolidated to a specific weight of 65 lb per cu ft. On such a basis, the void ratio will be 60% and therefore the actual water-holding capacity of the reservoir will never be reduced by more than 40% of the original capacity.

The specific yield of the deposit will depend on the composition, but it appears safe to guess that, for long-term withdrawals, it should be equal to at least half the porosity. Therefore, effective storage capacity of 30%, or about 7,500,000 acre-ft, plus several million acre-feet of open storage provided by the gates above spillway crest, will always be available.

This volume of storage combined with that of proposed reservoirs upstream will be fairly adequate, even assuming that all the reservoirs are filled and operating as ground-water reservoirs some 250 years hence—the runoff being diminished by that time (through Upper Basin consumptive use and diversions) to about 50% of the water supply currently available at Boulder Dam. When Boulder Dam and other surface reservoirs become ground-water reservoirs, the very large evaporation losses that now occur will be avoided. Such conservation may be considered very desirable practice in succeeding generations. In any event, practicable solutions of the silt problem will be available to engineers of the future, and present-day developments dependent on regulation of the Colorado River are not necessarily doomed to eventual extinction.

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[98] Cons. Engr., Piedmont, Calif.

[98a] Received by the Secretary December 5, 1945.

[99] *Transactions*, Am. Soc. C. E., Vol. 100 (1935), p. 307.

[100] *Ibid.*, Vol. 101 (1936), p. 261.

AMERICAN SOCIETY OF CIVIL ENGINEERS

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DISCUSSIONS

THE EFFECT OF BLANKETS ON SEEPAGE THROUGH PERVIOUS FOUNDATIONS

Discussion

By Preston T. Bennett

Preston T. Bennett,[16] Esq.[16a]—The importance of anisotropic permeability, resulting from stratification of natural deposits is emphasized clearly in Mr. Cedergren's discussion. If the symbols k_b and k_f are interpreted properly, as vertical and horizontal permeabilities,[16b] no further correction for anisotropy needs to be made to bring the formulas to a degree of accuracy greater than that obtainable by graphic methods.

As Mr. Cedergren has stated, considerable skill and experience in the construction of flow nets are necessary before such graphic solutions can be relied on. The writer's dissatisfaction with his own efforts at graphic solution caused him to use electric conduction models, and his observation of the nearly straight-line equipotential lines obtained from models of blanket systems led, in turn, to the opinion that an analytical solution for simple blanket problems was entirely feasible and less time consuming than either of the other methods.

A blanket over a more pervious stratum constitutes what might be termed a foundation with "coarse-grained" anisotropy. The horizontal coefficient of transmissibility for such a foundation is the ratio, $k_f\, z_f$; and, by analogy, the term "vertical coefficient of transmissibility" may be coined for k_b/z_b. Likening these coefficients to horizontal and vertical permeabilities k_I and k_{II}, whose transformation factor is $\sqrt{\frac{k_{II}}{k_I}}$ the comparable ratio $\sqrt{\frac{k_b}{z_b\, k_f\, z_f}}$ is obtained, which is the factor common to the solutions of blanket problems by W. H. Jervis, Assoc. M. Am. Soc. C. E., by Messrs. Barron and Lane, and by the writer.

Note.—This paper by Preston T. Bennett was published in January, 1945, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: March, 1945, by Reginald A. Barron; April, 1945, by V. A. Endersby; and June, 1945, by Harry R. Cedergren; and October, 1945, by W. J. Turnbull, and Kenneth S. Lane.

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[16a] Received by the Secretary November 16, 1945.

[16b] Correction for *Transactions:* In January, 1945, *Proceedings*, on page 30, after the definition for i, insert: "k = permeability; k_b = permeability of the blanket material, which influences the vertical seepage path; and k_f = permeability of the foundation material, which influences the horizontal seepage path."

The similarity of the factor a to an anisotropic transformation factor is mentioned to emphasize again the writer's opinion that no second transformation of dimensions is necessary if k_b and k_f in his notation are interpreted properly. This similarity also suggests the definition "coefficient of blanketed transmissibility" for the hitherto undesignated quantity a.

Messrs. Barron and Lane have both presented solutions for the uniform blanket which contain the term $(L_2 - L_1)$. This is equivalent to x_d of the paper. Inclusion of the term x_d eliminates the necessity for a separate equation and computation to determine values such as h_n in Eq. 9*a*.

Mr. Lane was not entirely correct in assuming that the writer's h_n was as defined in Fig. 8, but the modification of Eq. 9*a* as indicated in Eq. 29 is necessary if it is desired to generalize the blanket formula by adding factors to include flow under the impervious dam.

From Fig. 2 it is evident that

$$\frac{h}{\Delta h} = \frac{x_r}{x_r + x_d} \qquad (36)$$

Rewriting Eq. 9*d* in hyperbolic functions, the resistance for a blanket of length L_1 is

$$x_r = \frac{\sinh (a\, L_1)}{a \cosh (a\, L_1)} \qquad (37)$$

Referring to Fig. 8, the h and Δh in the paper correspond, respectively, to h_n and h in Mr. Lane's discussion. In Mr. Lane's notation, using $(L_2 - L_1)$ for x_d, Eq. 36 becomes

$$\frac{h_n}{h} = \frac{\dfrac{\sinh (a\, L_1)}{a \cosh (a\, L_1)}}{\dfrac{\sinh (a\, L_1)}{a \cosh (a\, L_1)} + (L_1 - L_2)} \qquad (38)$$

which reduces to identity with Eq. 31, and leads to the conclusion that the writer's use of two formulas, Eq. 9*a* and Eq. 36, is exactly equivalent to Mr. Jervis' solution.

Although the writer's use of the concept of blanket resistance introduces extra equations into the solutions, it actually simplifies the numerical work to be done. Consider, for example, the system shown in Fig. 14, which is similar to Fig. 8 except that a line of relief wells is used instead of the slot of infinite permeability. The uplift pressure at the toe will vary from a minimum at the wells to a maximum between wells, but it may be assigned an average value, h_{x2}, dependent on characteristics of the well system and the land-side flow pattern. If the average value h_{x2} is above the land-side surface area, the line of wells is equivalent in effect, at a distance beyond the radius of influence of the wells, to a slot of infinite permeability, as in Fig. 8, except that it acts as a source of flow from potential h_{x2} to the land-side surface at potential zero.[17]

[17] "Conference on Control of Underseepage," published by U. S. Waterways Experiment Station, Vicksburg, Miss. (see "Design of Relief Well Systems," by W. H. Jervis, p. 81; and "Comments on Design of Relief Wells," by P. T. Bennett, p. 87), April 1, 1945.

The solution of such a well and blanket system, involving a discontinuity of discharge at the line of wells, is considerably simplified by the use of blanket resistances as follows: Assume that a maximum permissible value for h_{x2} has been established. The curvilinear gradients A'B' of Fig. 14 can be replaced, for the computation of discharge, by the tangent gradients AW and WB if the resistances x_{r1} and x_{r3} are known. The slopes of these tangents, evident from Fig. 14, determine the foundation flows Q_1 $(= s_1\, k_f\, z_f)$ and Q_2 $(= s_2\, k_f\, z_f)$ on either side of the wells. The discharge of the well system required to reduce the land-side toe uplift pressure is $Q_1 - Q_2$. The size, spacing, and

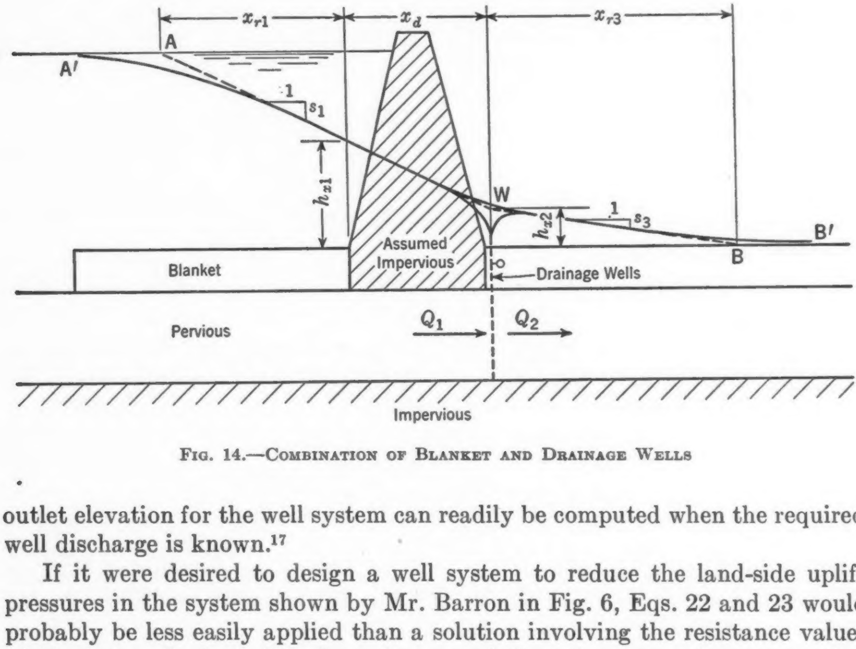


FIG. 14.—COMBINATION OF BLANKET AND DRAINAGE WELLS

outlet elevation for the well system can readily be computed when the required well discharge is known.[17]

If it were desired to design a well system to reduce the land-side uplift pressures in the system shown by Mr. Barron in Fig. 6, Eqs. 22 and 23 would probably be less easily applied than a solution involving the resistance values of blankets. In fact, Mr. Lane has noted that these equations are rather cumbersome for numerical work, even without the introduction of a drainage system.

Rewriting Eq. 22*c* with the origin at $x = L_3$, $x' = 0$:

$$h_{x'} = B' \left(e^{ax'} + e^{-ax'}\right) \quad \text{(39)}$$

in which B' is a factor similar to B in Eq. 22*c*. Thus, the resistance of the interval between L_2 and L_3 in Fig. 6 is

$$x_{r3} = \frac{h_{x'}}{\dfrac{d(h_{x'})}{dx'}} = \frac{e^{ax'} + e^{-ax'}}{a\left(e^{ax'} - e^{-ax'}\right)} \quad \text{(40)}$$

Fig. 15 is Mr. Barron's Fig. 6 redrawn with self-explanatory changes in notation which are made for easier use in resistance formulas. In the notation

of Fig. 15, from Eqs. 9*d* and 40,

$$x_{r1} = \frac{\sinh (a\, x_1)}{a \cosh (a\, x_1)} \qquad (41a)$$

and

$$x_{r3} = \frac{\cosh (a\, x_3)}{a \cosh (a\, x_3)} \qquad (41b)$$

From Fig. 15,

$$h_{x1} = \frac{h\, (x_{d1} + x_{r3})}{x_R} \qquad (42a)$$

and,

$$h_{x2} = \frac{h\, x_3}{x_R} \qquad (42b)$$

and, from Eq. 39,

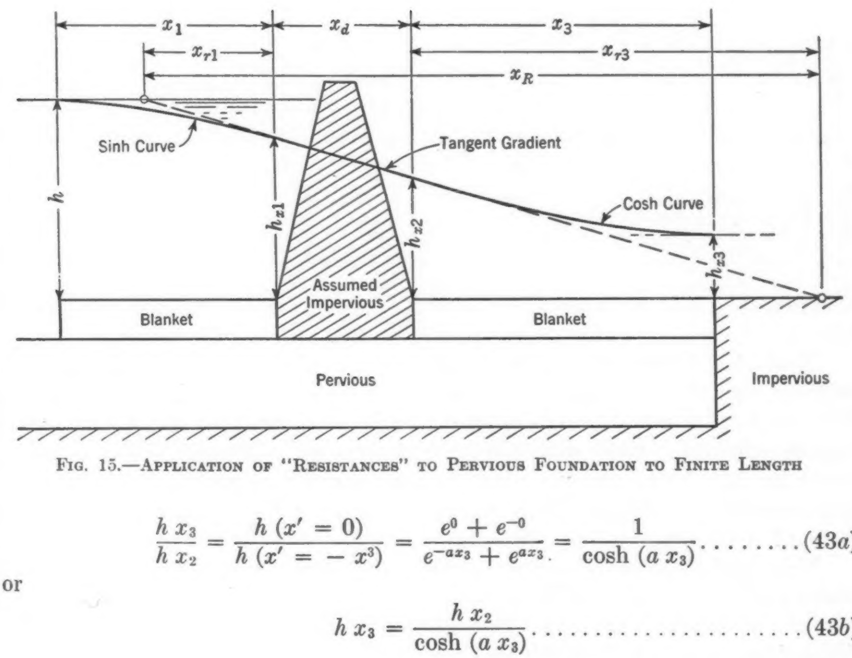


FIG. 15.—APPLICATION OF "RESISTANCES" TO PERVIOUS FOUNDATION TO FINITE LENGTH

$$\frac{h\, x_3}{h\, x_2} = \frac{h\, (x' = 0)}{h\, (x' = -\, x^3)} = \frac{e^0 + e^{-0}}{e^{-a x_3} + e^{a x_3}} = \frac{1}{\cosh (a\, x_3)} \qquad (43a)$$

or

$$h\, x_3 = \frac{h\, x_2}{\cosh (a\, x_3)} \qquad (43b)$$

The transformation of Eqs. 22 and 23 into the forms given in Eqs. 41, 42, and 43 seems to render them more useful in numerical applications.

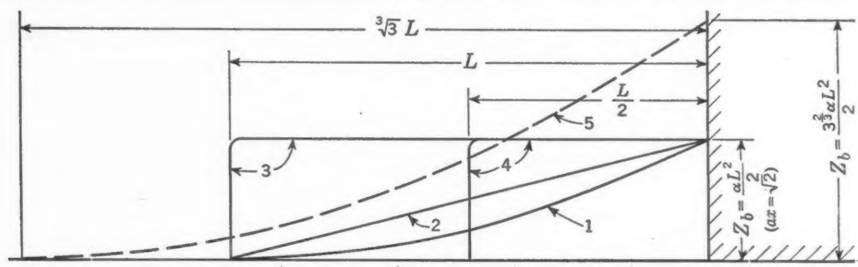
Quite properly, Mr. Lane has questioned the writer's comparison of uniform and triangular blankets. The conclusion (paragraph containing Eqs. 12, 13, and 14, last sentence) that a triangular blanket of maximum thickness z_b and length x is equivalent to a uniform blanket of the same thickness and length $x/2$ is obviously untrue if k_b were zero. In the latter case, the resistances would be x and $x/2$, respectively. The writer's comparison was made on blankets of optimum dimensions, such that $a\, x = \sqrt{2}$. These conclusions may be illus-

trated by Table 3, in which five blanket sections are superimposed for comparison of end areas and resistances. Blankets No. 1 and No. 5 are parabolic sections (see diagram in Table 3) described by

$$z = \frac{\alpha x^2}{2} \dotfill (44)$$

Eq. 44 can be shown analytically to represent the curve section that provides maximum resistance for a fixed-end area. The uniform blanket of length L

TABLE 3.—COMPARISON OF BLANKET SECTIONS



Description	Section 1	Section 2	Section 3	Section 4	Section 5
Comparative end area	1/3	1/2	1	1/2	1
Resistance in terms of L	0.5	0.5 to 0.63	0.63	0.44	0.72
Ratio, resistance to area	1.5	1.0 to 1.26	0.63	0.88	0.72

(Section 3, Table 3) is also of optimum proportions, as can be noted from Fig. 3.

Table 3 indicates some advantages of the vertically curved and triangular sections over the uniform section, and is the basis for the writer's assumption that sections of equal area, shaped similarly to Sections 2 and 4, are about equally effective. Even with careful determination of permeabilities as advocated by Mr. Turnbull, the best possible estimate of the factor a will not represent field conditions closely enough to justify hairsplitting in the selection of blanket shapes and dimensions.

This opinion is not to be interpreted as nonconcurrence with Mr. Turnbull's statements that field conditions should be determined with the greatest care, for only when this is done can the formulas developed in this paper and in the discussions be used to best advantage.

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DISCUSSIONS

COLUMN FORMULAS

Discussion

BY WILLIAM R. OSGOOD

WILLIAM R. OSGOOD,[43] M. AM. SOC. C. E.[43a]—Although the paper seems to have been written so that it could be understood, it evidently was not written so that it could not be misunderstood.

The paper contemplates only column failure as distinct from local failure and from crushing of the material, and it contemplates only central loading. Before account can be taken of accidental eccentricities, initial curvatures, and that sort of thing, it seems desirable to eliminate the effects of these disturbing factors so far as possible. When a resulting basic column curve has been obtained, the effects of accidental imperfections in modifying the curve may be considered.

Some of the discussers consider the secant formula to be a rational one. The secant formula is simply the familiar formula,

$$f = \frac{P}{A} + \frac{M\,c}{I} \qquad (64a)$$

written for columns,

$$f = \frac{P}{A} + \frac{P\,e \sec \frac{l}{2\,i}\sqrt{\frac{P}{E\,A}}}{I}\,c \qquad (64b)$$

Eqs. 64 express accurately the relations between the variables that enter so long as Hooke's law applies. For slight departures from Hooke's law, there will be slight inaccuracies. At failure, of either a beam or a column of ordinary structural material, Hooke's law no longer applies even approximately; the inaccuracies are great, and a rational basis can no longer be claimed for the formulas. The computed value of f at failure has no physical meaning whatsoever. In the case of a simple beam ($P = 0$ in Eq. 64a), the computed

NOTE.—This paper by William R. Osgood was published in December, 1944, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: February, 1945, by L. E. Grinter, George Winter, and Jonathan Jones; March, 1945, by Edward Godfrey; May, 1945, by Ralph E. Spaulding, and James H. Cissel; and June, 1945, by Leon Beskin; and November, 1945, by Louis Balog.

[43] Materials Engr., National Bureau of Standards, Washington, D. C.

[43a] Received by the Secretary December 13, 1945.

value of f will vary widely with the shape of the cross section, for the same material. In the case of a column, the computed value of f will vary widely with the shape of the cross section, with the eccentricity, and with the length. Likewise, with a given value of f, the computed values of $\frac{P}{A}$ will bear little relation to the actual values of $\frac{P}{A}$ at failure of columns of different lengths, eccentricities, and shapes of cross section, but of the same material. An unknown, variable factor of safety results when design is based on Eq. 64*b* with a constant f.

The view that the secant formula is rational seems similar to maintaining that Rankine's formula is rational, as was often done in the past. Frequently, the assumptions of a theory are not ideally satisfied; but, for the theory to be rational, the assumptions must be reasonably well satisfied. The assumptions underlying the secant formula, considered as a rational one, are not reasonable, and the formula does not and cannot in general produce results in agreement with tests. For example, a series of tests made recently has shown that, for eccentricity ratios of $\rho = \frac{e\,c}{i^2}$ up to 1, the formula,

$$\lambda = \frac{\lambda_o}{1 + 4\,\rho\,\sigma^2} \qquad \text{(65a)}$$

fits the test results well. In Eq. 65*a*,

$$\lambda = \frac{1}{\pi}\frac{l}{i}\sqrt{\frac{S}{E}} \qquad \text{(65b)}$$

in which l is the length of the eccentrically loaded column. In the secant formula,

$$\lambda = \frac{2}{\pi\sqrt{\sigma}}\sec^{-1}\left(\frac{1}{\beta\,\sigma} + C\right) \qquad \text{(65c)}$$

β and C cannot be determined to fit these test results at all accurately, except possibly for a narrow range. Much less are β and C related to properties of the material. The only rational consideration of the strength of eccentrically loaded columns is along the lines pursued by some of the authors cited by Mr. Balog, notably Karl Ježek and E. Chwalla.

All this does not infer that a formula of the secant type may not be useful, considered as an empirical formula.

A quick and easy formula is demanded by some of the discussers. In comment the writer would quote Mr. Beskin:

> "* * * there is no practical * * * objection to the use of a limited number of simple formulas, instead of one complicated formula covering the entire range of possible slenderness ratios."

To cover the entire range accurately with one or even two simple formulas is asking a lot. It will rarely be possible, depending, of course, on one's ideas of simplicity.

Some object to a formula, like the secant formula, that expresses the ratio of slenderness explicitly as a function of the stress, rather than the stress as a function of the ratio of slenderness. In practice both the load and the length are given. In a formula is it unimportant which one is expressed in terms of the other?

Dean Grinter's criticism indicates the real problem—on the one hand, the need for re-examining methods from time to time, and, on the other hand, the need for keeping them sufficiently simple for use by the ordinary designer. Dean Grinter raises a practical question that must be faced when he says that "* * * much design work is actually performed by persons of limited competency in structural design." The final answer might be to increase the competence of designers! Whether the immediate answer is to confine the use of column formulas to the Rankine formula and the straight-line formula the writer does not know, especially in view of some of the complicated formulas in use by structural designers in the field of aircraft.

Professor Winter and Mr. Beskin suggest that a discussion of the secant yield strength, S, would be of interest. This question has been raised before. It has been answered partially, in reference to beams, in another paper.[44] To answer it for columns, one may write the double-modulus column formula,

$$\sigma = \frac{1}{\lambda^2_o} \frac{\bar{E}}{E} \qquad (66)$$

and ask under what conditions Eq. 66 represents the same reduced column curve for different materials. In Eq. 66

$$\bar{E} = \frac{E' \, I' + E \, I}{I_g} \qquad (67)$$

in which:

E' is the tangent modulus of elasticity of the material at the stress $s = P/A$;
I' is the moment of inertia about the axis of average stress of the part of the cross-sectional area that is subjected to an increase of stress at the instant of failure of the column;
I is the moment of inertia about the axis of average stress of the part of the cross-sectional area that is subjected to a decrease of stress at the instant of failure; and
I_g is the moment of inertia of the cross-sectional area about a gravity axis parallel to the axis of average stress.

The position of the axis of average stress is defined by the relation $E' \, Q' = E \, Q$, in which Q' and Q are the statical moments about the axis of average stress of the two parts of the cross-sectional area just mentioned.

If, now, for two materials, Nos. 1 and 2, and for geometrically similar cross sections, the column curves are defined by:

$$\sigma_1 = \frac{1}{\lambda^2_1} \frac{\bar{E}_1}{E_1} \qquad (68a)$$

[44] "A Rational Definition of Yield Strength," by William R. Osgood, *Journal of Applied Mechanics*, Vol. 7, June, 1940, p. A-61.

and

$$\sigma_2 = \frac{1}{\lambda^2_2}\frac{\bar{E}_2}{E_2}\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(68b)$$

—what conditions are required to make these two curves the same? For any value of $\sigma_1 = \sigma_2$, the corresponding value of λ_1 is equal to λ_2 if $\bar{E}_1/E_1 = \bar{E}_2/E_2$. From the definition of $\bar{E}$, and the axis of average stress, this is the same as requiring that $E'_1/E_1 = E'_2/E_2$; or

$$\frac{1}{E_1}\frac{ds_1}{de_1} = \frac{1}{E_2}\frac{ds_2}{de_2}\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(69)$$

in which e is the compressive strain at the stress s. Since $\sigma_1 = \sigma_2$,

$$\frac{s_1}{S_1} = \frac{s_2}{S_2}\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(70)$$

and

$$\frac{1}{E_1}\frac{ds_1}{de_1} = \frac{1}{E_2}\frac{S_2}{S_1}\frac{ds_1}{de_2}\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(71)$$

or, upon integration,

$$\frac{E_1\,e_1}{S_1} = \frac{E_2\,e_2}{S_2}\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(72)$$

The stresses and strains of the two materials must be related as expressed by Eqs. 70 and 72.

Eqs. 70 and 72 must apply in particular when $\sigma_1 = \sigma_2 = 1$—that is, when s_1 and s_2 are equal to the yield strengths of the two materials. If e_{11} and e_{22} denote the strains corresponding to the yield strengths, Eq. 72 becomes, for this case:

$$\frac{S_1/e_{11}}{S_2/e_{22}} = \frac{E_1}{E_2}\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(73)$$

which states that the slopes of the lines through both the origins and the yield strengths of the stress-strain curves must be proportional to the moduli of elasticity of the two materials. This requires that the yield strengths of the two materials be determined as the stresses at the intersections with the stress-strain curves of secants through the origin having slopes equal to $m\,E_1$ and $m\,E_2$, in which m is an arbitrary constant $0 < m < 1$—hence, the name "secant yield strength" for yield strengths so determined.

(In a conversation with the writer, Nathan M. Newmark, M. Am. Soc. C. E., has pointed out that, since $E'_1/E_1 = E'_2/E_2$, at the yield strength, $E'_{11}/E_1 = E'_{22}/E_2$ and therefore the yield strengths may be determined as the stresses at which the slopes of the stress-strain curve are $n\,E_1$ and $n\,E_2$, with $0 < n < 1$. This is probably the first time that "Johnson's elastic limit" has been justified on any rational basis. Since it is the slope of the stress-strain curve that is particularly significant in compression members, it may be that Johnson's elastic limit is a better measure of the yield strength than the secant yield strength. However, Johnson's elastic limit does require greater care in its determination than does the secant yield strength.)

The necessary and sufficient conditions, therefore, that Eqs. 68 represent the same curve are that Eqs. 70 and 72 be satisfied at every point and that S_1 and S_2 be secant yield strengths determined with the same value of m. It will always be possible to determine S_1 and S_2 as required. Whether Eqs. 70 and 72 are satisfied at every point depends on the materials considered. Many tests have shown, however, that, for nominally similar materials, such as materials complying with a given specification, Eqs. 70 and 72 are in fact satisfied to a high degree of approximation, even when the yield strengths differ widely.

Finally, the generalization from two materials to any number of materials is obvious, the practical point being that the yield strengths must be determined as secant yield strengths with slopes of the secants equal to $m\,E$, in which m is the same constant for all the materials being considered and E is the modulus of elasticity of each individual material.

Incidentally, whether Eqs. 70 and 72 are satisfied or not, the scatter in the column data can be reduced by plotting σ against λ_o rather than s against l_o/i, provided S is the secant yield strength. Moreover, no other method is likely to give as good results consistently; and no other method can give as good results if Eqs. 70 and 72 are satisfied at every point and if S is the secant yield strength.

In Mr. Godfrey's opinion, "The Gordon-Rankine formula has limited application. This formula is fraught with danger, if it is used in the range of slender columns." One of the objectives of the paper was to call attention to this kind of danger. If the Rankine formula is written in the form of Eq. 3*b*, in which $B \geqq 0$, the average stress can never exceed the Euler value. If B is chosen to give safe values in the range of "medium" ratios of slenderness, safe values will be obtained for slenderer columns also. The writer agrees with Mr. Godfrey that a factor of safety of 5 in the Euler range is excessive. If a variable factor of safety is used (and it probably should be used in some form), it is the opinion of the writer that the factor should be less for short and for long columns than for columns of medium length, for which λ_o is usually about 1, depending on how S is measured.

In its most general, dimensionless form, Mr. Spaulding's first formula, Eq. 35, may be written

$$\sigma = \frac{1}{\lambda^2_o}\,\frac{\lambda^4_o + \beta\,\lambda^2_o + \gamma}{\lambda^4_o + \delta\,\lambda^2_o + \epsilon} \qquad (74a)$$

and his second one, Eq. 37,

$$\sigma = \frac{\beta + \lambda^2_o}{\gamma + \delta\,\lambda^2_o + \lambda^4_o} \qquad (74b)$$

(Mr. Spaulding's σ_y—a yield strength—is not to be confused with the writer's σ, which is a dimensionless "stress".) Eq. 74*a* contains four arbitrary constants; and Eq. 74*b*, three. These are too many to determine conveniently and more than are necessary. Mr. Spaulding arbitrarily takes $\beta = \gamma + 1 = \delta = \epsilon + 1$ in Eq. 35 and $\beta = \gamma = \delta$ in Eq. 37, thus reducing the number of constants to one in each formula. In terms of λ_o and σ, the solid curves of Fig. 2(*a*) reduce to one curve.

Laboratory conditions differ from conditions in engineering structures primarily in that the former conditions are usually better known than the latter. Undoubtedly, the most troublesome unknown is k. Mr. Spaulding and Mr. Beskin consider this factor a variable rather than a constant. Perhaps it is better called a parameter. It is usually exceedingly difficult (that is, particularly time consuming) to determine k rationally for a given structure. It seems that k must be determined largely by judgment, although the excellent studies of F. Bleich, M. Am. Soc. C. E., may serve as a helpful guide. The writer is of the opinion that conditions such as unintentional eccentricities and initial curvature are best taken care of by the factor of safety.

Professor Cissel lists four possible modes of failure. The first, compressive failure by crushing the material, is important for "brittle" materials—in structural work, primarily for concrete. It was important for cast iron and may become so again for metals, with the development and use of alloy steels, alloys of aluminum, and alloys of magnesium, for example. The formulas of the paper were concerned implicitly with column failure, Mr. Cissel's second mode of failure. His third mode of failure, twisting, is sometimes lost sight of but must definitely be considered; and perhaps the fourth mode, local failure, is the type most likely to occur, certainly in short columns.

The objection to using, instead of a formula, an averaged curve of experimental results (which Mr. Beskin suggests as a possibility) is that such a curve cannot be transferred readily. No two users would be dealing with the same curve, although a sufficient number of corresponding abscissas and ordinates might be read from a master curve and a list of these points published so that practically the same curve would be drawn by everyone.

It is true, as Mr. Beskin states, that a decreasing function is always a monotonically decreasing function; but a monotonically decreasing function is not always everywhere a decreasing function.

The writer disagrees with Mr. Beskin that the double-modulus theory involves a basic error in theory, in that it presupposes a theoretically straight column. In carefully made and carefully centered columns, if the departures from the ideally assumed conditions were small but sufficiently great to give results differing significantly from those predicted by the theory, then the theory would be in error. Actually, no such significant differences in results are obtained. It is presumably the column that is in error, not the theory; that is, the imperfections in the column lead to results not in complete accord with those predicted by the theory.

When the writer stated "It would be very hard to justify more than two constants of the material in any practical column formula," he had in mind the dimensionless constants of the formulas written in terms of λ_o and σ. As Mr. Beskin states, the formulas in terms of l_o/i and P/A all contain one more constant—namely, E, the modulus of elasticity of the material.

To choose for the constant S the intercept S_o of the column curve with the axis of P/A, as Mr. Beskin would prefer, sounds easier than it is. It is not so easy to determine this intercept. A column of zero length has practically infinite strength; and, if S_o is to be determined by extrapolation, extrapolation of what? If the material tested is all uniform and has the same mechanical

properties, and if the cross sections of all specimens are geometrically similar, it may be possible by extrapolation from low values of l_o/i to arrive at a fairly definite value of S_o for that material and that cross section. This value of S_o has no significance for a different, but nominally the same, material and a different shape of cross section.

Mr. Beskin calls attention to the effect of the shape of cross section on column strength. The effect of shape is illustrated in Fig. 8, in which $\bar{E}/E$ is shown as a function of E'/E for a number of cross sections.[4] Since E' is the slope of the stress-strain curve—that is, the tangent modulus at a particular stress—the abscissas also represent stresses to some scale. For a given value of E'/E that is not too small, the spread in $\bar{E}/E$ for practical symmetrical sections is seen not to be excessive. The same is not true of unsymmetrical sections, where large variations in $\bar{E}/E$, and thus in the strength (Eq. 66), may be obtained.

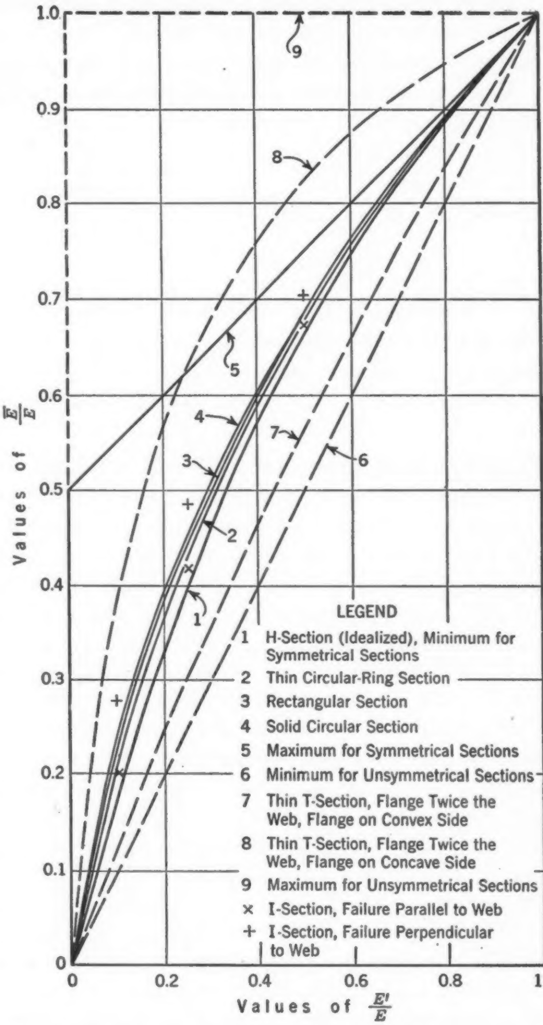

FIG. 8.—THE EFFECT OF THE SHAPE OF THE CROSS SECTION ON THE STRENGTH OF COLUMNS

Mr. Beskin regrets that the dimensions of the constants in Eqs. 3 to 21 are not the same throughout all the formulas, but he does not say why. Perhaps he would have liked the dimensionless constants of the first forms, Eqs. 3*a* to 21*a*, to have been retained in the second forms, Eqs. 3*b* to 21*b*; but it seemed to the writer that this would merely clutter up these formulas unnecessarily. The constants are not intended to have the same values in each formula. Thus, whether Eq. 13 gives values less than the Rankine formula depends on the values of B and C in Eq. 13, on the one hand, and on the value of B in Eq. 3 on the other hand. There is no connection be-

[4] "Column Curves and Stress-Strain Diagrams," by William R. Osgood, *Journal of Research*, National Bureau of Standards, Vol. 9, October, 1932, p. 571.

tween the value of B in Eq. 13 and the value of B in Eq. 3. The writer regrets the confusion that might have been avoided by using different letters for the constants in all the formulas.

As for the necessity for a cutoff when Eqs. 15 and 18 are used, it may be stated in general that any formula may need a cutoff if the material fails at a lower stress than the column stress. Cutoffs may also be needed to take account of crinkling failure without bending of the column as a whole.

Eqs. 15 and 18 were derived by methods published elsewhere.[4] When the column strength is represented by

$$\sigma = \frac{1}{\lambda^2_o} \frac{E'}{E} \quad \text{......................(75)}$$

and, when

$$K\,n \left(\frac{S}{E}\right)^{n-1} = \beta \quad \text{......................(76a)}$$

Eq. 17 leads to Eq. 15*a*. When the column strength is represented by Eq. 66 with $\bar{E}$ the double modulus for an idealized H-section (negligible web); and, when

$$\frac{1}{2} K\,n \left(\frac{S}{E}\right)^{n-1} = \beta \quad \text{......................(76b)}$$

Eq. 17 also leads to Eq. 15*a*. When, in Eq. 66, $\bar{E}$ is the double modulus for a rectangular cross section, and β is defined by Eq. 76*a*, Eq. 17 leads to Eq. 18*a*.

Both Eqs. 15 and 18 were intended to be purely empirical column formulas, with the β's and n's to be determined as the results of tests. On the other hand, Eq. 75 and the resulting formula, Eq. 15*a*, with β in effect given by Eq. 76*a*, have received wide experimental confirmation in ordinary, carefully made laboratory tests for a wide variety of structural materials. The writer would feel much more confident in predicting column strength of a new material from Eq. 75, Eq. 15*a* and Eq. 76*a*, or Eq. 65*a*, than from the secant formula.

Mr. Balog's "ideal conditions" (second paragraph of his discussion), leading to a column curve composed of Euler's curve and a horizontal line (Fig. 5), include of necessity an "ideal" compressive stress-strain curve composed of a modulus line and a horizontal yield line. Most structural materials do not have so simple a stress-strain curve; in particular, they do not have the sharp knee of Mr. Balog's ideal material. The blunter the knee, the wider the gap will be between the "ideal" column curve and the actual column curve.

Mr. Balog writes that an infinite variety of double-modulus curves is possible between the proportional limit and yield stress for the same steel. If the steel is really the same, only one double-modulus curve is possible, and the writer believes Mr. Balog is mistaken, unless he means that an infinite variety is possible depending on the shape of the cross section; but, as noted previously, the column strength does not vary very much with different symmetrical shapes in common use.

The writer was not aware that he made any "summary assumptions for the buckling lengths." The free length, or the buckling length, is one of the most difficult variables, if not the most difficult, to evaluate in the design of columns; and the writer is just as much against summary assumptions in this case as is Mr. Balog.

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DISCUSSIONS

THE SAFETY OF STRUCTURES

Discussion

By F. H. Frankland, and Elliott B. Roberts

F. H. Frankland,[27] M. Am. Soc. C. E.[27a]—The structural designer is concerned with the solution of probability problems by the consideration of the various factors governing his design, such as safety, use characteristics, economy, future suitability, and durability. Although, in recent times, structural engineering has undergone changes at an accelerated pace, structural engineers have not generally kept pace with the rapid advance made in some other branches of engineering.

The author states, in the "Synopsis," that: "The word 'resistance' is a general term indicating 'strength' or capacity to resist failure." It is important that, in discussing safety factors, this use of the term "resistance" should receive general acceptance and understanding.

The safety of a design is not determined by the use of an average factor of safety but is governed by the safety factor at the weakest part. However, in most structural systems there is often a redistribution of stress, under load, throughout the system which makes the resulting safety factor greater than that of any one part:[28]

> "In practically all cases a structure designed by the use of conventional methods, with a high safety factor, will be less safe than one designed by the use of more refined methods and using a smaller safety factor. The conventional safety factor idea is that the safety factor for a particular design under given loading conditions is a constant coefficient; but actually the safety factor varies."

The skill of the designer is indicated, in part, by his understanding of the degree to which elasticity and plasticity influence the behavior of a structure because of geometrical displacements under load in the various parts and the consequent possible creation of multiaxial and other secondary stresses.

Note.—This paper by Alfred M. Freudenthal was published in October, 1945, *Proceedings*.

[27] Cons. Engr., New York, N. Y.

[27a] Received by the Secretary November 23, 1945.

[28] "Modern Stress Theories," by A. V. Karpov, *Transactions*, Am. Soc. C. E., Vol. 102 (1937), p. 1207.

The author states (under the heading, "The Factor of Safety"):

> "With increasing perfection of design methods the element of 'ignorance' can be largely eliminated; but the element of 'uncertainty' is caused by circumstances that can be changed, to a certain extent, but can never be removed."

Nevertheless, little real betterment of design methods can be achieved by "refinements" in the application of mathematical theories unless it is accompanied by an intelligent understanding of the behavior of structures in use. Such refinements mean little when basic assumptions are wide of reality. It is of prime importance that engineers realize that there is but little excuse for the element of so-called ignorance and that the safety factor is a matter governed mainly by the degree of uncertainty. Generally accepted design methods too often neglect stress concentrations and the effects of fatigue and consider that materials possess perfectly elastic properties. It should be remembered that, for high-alloy structural steels, such as 18–8 stainless and nonferrous structural alloys like aluminum and magnesium, the elastic limit is indefinite and the stress-strain curve is continuous. The general and exclusive use of the elastic theory in the design of structures is partly responsible for the lack of understanding among many engineers of the actual behavior of structural materials. The "aspect of practical application" which the author refers to in "Part 1.—General Principles: Introduction" should be clear in the mind of the designer at all times. As a consequence of the better understanding of the behavior of structures and structural materials, referred to by the author, some of the generally accepted underlying concepts of structural design are inadequate.

Recent investigations of stress distribution in structures under load clearly justify dissatisfaction with design methods which have hitherto been accepted as adequate. In this connection it would be well to study the paper "Theory of Elastic Stability Applied to Structural Design."[29]

An understanding of the theory of limit design will explain the capacity of a structural material to sustain occasional overstrain without damage to the ultimate endurance limit from the design point of view. The term "endurance limit" should not be confused with "fatigue strength." The endurance limit may be defined as the fatigue stress under complete stress reversal below which metal can withstand an infinitely large number of stress reversals without failure.

Any loaded structure will behave in a manner inconsistent with the elastic theory in that various parts and connections may be locally overstressed, sometimes beyond the local yield point of the material, thus causing plastic flow and a consequent redistribution of stress at the affected parts. The elastic theory is based on the assumption that the behavior of all parts of the structure is entirely elastic at all times when not loaded beyond the safe allowable design stresses. This assumption is obviously untrue as it ignores the element of uncertainty involved in differentials of stress in various parts of riveted,

[29] "Theory of Elastic Stability Applied to Structural Design," by Leon S. Moisseiff and Frederick Lienhard, *Transactions*, Am. Soc. C. E., Vol. 106 (1941), p. 1052.

bolted, or welded construction, as well as conditions of locked-up stresses and local variations in the yield point of the material. These variations and constraints are "ironed out" by the application of service loads, and only then may a structure behave in accordance with the concepts of the elastic theory. However, should a structure be further loaded beyond this point of stress redistribution, the limit theory must be resorted to in order to care for actual conditions. In this connection consideration must be given to what Raymond J. Roark calls "damaging stress,"[30] which is:

> "* * * the least unit stress, of a given kind and for a given material and condition of service, that will render a member unfit for service before the end of its normal life. It may do this by producing excessive set, or by causing creep to occur at an excessive rate, or by causing fatigue cracking, excessive strain hardening, or rupture."

The designer should also recognize what may be called the factor of utilization, which is the ratio of the allowable stress to the ultimate strength. Where the stress is proportional to the load, this factor is the reciprocal of the safety factor. Designers in the aeronautical field use the term "margin of safety" to express the percentage by which the ultimate strength exceeds the design load.

Plasticity is a most valuable characteristic in any structural material when the material also possesses elasticity. If structural members did not have the ability to accommodate themselves to the redistribution of stress through this characteristic of plasticity, many bridges and buildings would not stand up no matter what numerical safety factor was used in their design by the elastic theory.

There is a distinct trend among structural engineers toward the evaluation of adjustments necessary to bring the idealized conditions embraced within the elastic theory into agreement with the realism of actual conditions as comprehended in the limit theory of design.

These comments are not offered in a spirit of criticism, but with the purpose of calling the attention of engineers to the desirability of understanding what is the true strength of a structure, and, above all, to the desirability of adopting a realistic approach to design problems.

Elliott B. Roberts,[31] M. Am. Soc. C. E.[31a]—Although the validity of this paper is not questioned, it is nevertheless regrettable that the author failed to introduce a discussion of earthquake factors.

Earthquakes are frequent in certain well-recognized zones. What is not, perhaps, so generally recognized is that they may, and do, occur almost anywhere. No one could safely state that any place is perfectly free from the danger of a destructive earthquake. Evaluation of the risk in a specific location may introduce a delicate problem; it should never be ignored completely.

[30] "Formulas for Stress and Strain," by Raymond J. Roark, McGraw-Hill Book Co., Inc., New York and London, 2d Ed., 1943, p. 5.

[31] Lt. Comdr.; U. S. Coast and Geodetic Survey, Washington, D. C.

[31a] Received by the Secretary November 15, 1945.

The subject of earthquake factors has been treated adequately by the American Standards Association,[32] and a more detailed treatment is contained in "Uniform Building Code."[33] The latter publication, of course, is the result of extensive experience gained by constructors in the most seismic areas of the United States. Experience in these areas is supported by extensive field observations and analysis of ground and building vibrations made by the United States Coast and Geodetic Survey.

Corrections for *Transactions:* In October, 1945, *Proceedings*, on page 1162, in Eq. 6*a*, change "η" to "s_a" and, in Eq. 6*b*, change "η" to "s_r"; on page 1168, line 14, change "fluctuation for" to "fluctuation by" and line 39 delete the comma after "organization"; on page 1171, the last line on the page, delete the comma after "ordinates" and change "or" to "and"; on page 1172, line 1, change "the specific" to "specific"; on page 1177, line 24, delete the comma after "spans"; on page 1178, line 2, change "*I* and *n*" to "*I* in *n*"; on page 1179, line 23, change "less than" to "larger than"; on page 1185, change line 18 to read "without the final endurance limit being affected, to the maximum permissible"; on page 1188, change line 5 to read "— and applying Eqs. 17 and 18:"; on page 1189, line 22, change "$\frac{1}{\eta} S_{fo}$" to "$\frac{1}{\eta} s_{fo}$"; and, on page 1190, in line 10, change "for instance, the" to "for which the" and change line 11 to read "of the tensile strength s_t of silicon steel was established (Fig. 3), the."

[32] "American Standard Building Code Requirements for Minimum Design Loads in Buildings and Other Structures," ASA—A58.1—1945, pp. 11 and 12.

[33] "Uniform Building Code," Pacific Building Officials Conference, Los Angeles, Calif., 1937.

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DISCUSSIONS

MOVING LOADS AND RESTRAINED BEAMS AND FRAMES

Discussion

BY FREDERICK S. MERRITT, ALFRED B. HEISER, AND T. B. RIGHTS

FREDERICK S. MERRITT,[31] Assoc. M. AM. Soc. C. E.[31a]—Inevitably, in practice, simplifications of theoretical solutions will occur to the designer. Such simplifications of Professor Brumfield's method, made by the writer as a result of experience with practical design problems, have been so numerous that only the basic assumptions have been untouched. For the present discussion Professor Brumfield's "transmission coefficients" may be compared with the writer's "moment ratios."[32]

The action of a beam in a continuous structure depends upon its shape and size relative to the remainder of the structure. Constants evaluating these relative shape and size characteristics may be selected and defined almost at will; but, of course, if they are to be of practical value, they should be few in number and sufficiently descriptive of the action of the member. For the purpose of this discussion, three fundamental constants—K, $m^F{}_R$, and $m^F{}_L$—will be used. Clockwise end moments and end rotations will be considered positive and counterclockwise moments and rotations negative.

Consider an unloaded member removed from a monolithic structure and simply supported instead. Then, K is the moment that must be applied at one end of this beam to produce unit rotation at the other end. Now, fix the right end of the member and apply a unit moment at the left end. For this condition, the moment that will be produced at the right, or fixed, end is $1/m^F{}_R$. Similarly, the carry-over ratio for a fixed end at the left is $1/m^F{}_L$. For beams of constant moment of inertia, $m^F{}_R = m^F{}_L = 2$ and $K = 6\,E\,I/L$.

The coefficients K and m^F are sufficient to describe the action of this member when it is once again returned to its position in the continuous structure.

NOTE.—This paper by R. C. Brumfield was published in May, 1945, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: October, 1945, by R. W. Stewart, Nathan D. Brodkin, and T. F. Hickerson; and November, 1945, by Max W. Strauss, D. A. Mackenzie, and Charles E. Schaffner; and December, 1945, by J. Kalb, G. Rolsma and F. W. Gieseking, Stanley S. Schure, and Alexander Dodge.

[31] Editorial Asst., *Engineering News-Record*, New York, N. Y.

[31a] Received by the Secretary November 20, 1945.

[32] "Moment Ratios Simplify Continuity Analysis," by Frederick S. Merritt, *Engineering News-Record*, November 1, 1945, p. 112 and November 15, 1945, p. 102.

Stiffness s of the end of an unloaded beam in a monolithic structure is defined as the moment producing a rotation of unity at the end where it is applied, when the other end of the beam is restrained against rotation by other members of the structure. For an unloaded member of a continuous structure the moment ratio m is defined as the ratio of the moment introduced at one end of the span to the moment carried over, or transmitted, to the other end.

Then, the stiffness of the right end of a beam can be shown to be:

$$s_R = \frac{K}{m^F{}_R - \dfrac{1}{m_L}} \quad \text{(71a)}$$

Similarly, the stiffness of the left end is:

$$s_L = \frac{K}{m^F{}_L - \dfrac{1}{m_R}} \quad \text{(71b)}$$

For beams of constant moment of inertia:

$$s_R = \frac{K}{2 - \dfrac{1}{m_L}} \quad \text{(72a)}$$

and

$$s_L = \frac{K}{2 - \dfrac{1}{m_R}} \quad \text{(72b)}$$

Eqs. 71 and 72 should be compared with Professor Brumfield's stiffness index formulas (Eqs. 3).

The restraint r at the end of an unloaded beam at a joint is defined as the moment applied at that end to produce a unit rotation in all the members of the joint.

Since, by definition of continuity, the end rotations of all the members of a joint are equal, restraint of the member under consideration is equal to the sum of the stiffnesses of the adjacent ends of the other members of the joint. Furthermore, the moment induced in any of these other members bears the same ratio to the applied moment as the stiffness of the member does to the restraint. Consequently, end moments are distributed at a joint in proportion to the stiffnesses of the members. These relationships should be compared with Professor Brumfield's restraint index formulas, moment splits, torque splits, and technique of distribution (Eqs. 9, 39, 40, and 41).

Moment ratios can be computed in terms of K and, through r, the moment ratios of preceding spans. It can be shown that:

$$m_R = m^F{}_R + \frac{K}{r_R} \quad \text{(73a)}$$

and

$$m_L = m^F{}_L + \frac{K}{r_L} \quad \text{(73b)}$$

For beams of constant moment of inertia,

$$m_R = 2 + \frac{K}{r_R} \quad \text{(74a)}$$

and

$$m_L = 2 + \frac{K}{r_L} \qquad \text{(74b)}$$

These formulas should be compared with Professor Brumfield's formulas for transmission coefficients (Eq. 5).

End moment formulas may be given in terms of moment ratios as follows:

$$M_R = -\frac{K\,(m_L\,\phi_R + \phi_L)}{m_R\,m_L - 1} \qquad \text{(75a)}$$

and

$$M_L = -\frac{K\,(m_R\,\phi_L + \phi_R)}{m_R\,m_L - 1} \qquad \text{(75b)}$$

in which ϕ is the end rotation under load considering the member as simply supported. Fixed-end moments may be obtained by setting the moment ratios in Eqs. 75 equal to $m^F{}_R$ and $m^F{}_L$, respectively. These formulas, also, should be compared with the corresponding ones given in the author's paper (Eqs. 14 and 27).

When the suggested comparisons are made, it may be seen that in several of the formulas the use of moment ratios decreases the number of arithmetical steps involved, particularly for the cases of variable moment of inertia. There is no arithmetical advantage in using transmission coefficients, which are reciprocals of moment ratios, since multiplication is as easy as division. Similarly, computation of stiffness as the ratio of two factors involves no more steps than that of stiffness index, which is the product of two factors. However, calculation of restraint as the sum of stiffnesses is considerably easier than that of restraint index as the reciprocal of the sum of reciprocals of stiffness indexes. Furthermore, there is a considerable advantage in moment distribution in using K, s, and r, instead of Q, i, and R, respectively, because the former bear direct relationships to the properties of the structure, the latter reciprocal. Thus, much work may be saved and a better visualization of structural action obtained by changing the reciprocal terms used by Professor Brumfield to direct ones.[32]

Alfred B. Heiser,[33] M. Am. Soc. C. E.[33a]—For a number of years the writer has searched among the indeterminate stress theories such as the Cross method and the method of fixed points to find a quick and comparatively easy method for computing indeterminate stresses; but disappointment seemed to result from these excursions until he encountered Professor Brumfield's theory as described in the present paper. Few, if any, of the theories make it possible to place loads definitely in order to obtain the maximum moments or shears either at the supports or at intermediate points in the span. It is also very difficult to decide by these theories just which spans must be loaded to obtain the maximum results. Professor Brumfield's paper fills this need. His method permits the effects at any point of a load or group of loads to be summed up, readily, at any other point throughout the entire structure however complicated the structure or grouping of loads may be.

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[33] Chf. Draftsman, Am. Can Co., New York, N. Y.

[33a] Received by the Secretary November 30, 1945.

Does this seem impossible? Not if one peruses the full theory developed by Professor Brumfield since his system has been developed to cover wind stresses, sidesway, beams of any type and shape, arches, and, best of all, structures having spans that are noncoplanar.[3] He has conquered stress analysis through three-dimensional space. The writer has personally checked many of the formulas for this advance theory and has found that, although they are complicated, they are workable.

In all cases, these formulas cover the general as well as the special cases. As the span member or load conditions are simplified, the equations are reduced to forms familiar to all—that is, the formulas used in ordinary beam design.

T. B. RIGHTS,[34] Assoc. M. Am. Soc. C. E.[34a]—This method is not new or untried. The Cooper Union, in New York, N. Y., has offered this course for graduate credit, since 1938. Professor Brumfield filed his paper, "Moving Loads on Beams with Restrained Ends," in the Engineering Societies Library in 1937. The task of placing a moving load for maximum moment at any point on a restrained beam is quite difficult by older methods. In the Hardy Cross method,[2] it meant that unit loads must be placed successively across the beam and their influence calculated, which is quite time consuming. By the use of Anger's tables,[35] it is comparatively simple to select positions for maximum moment. These tables are for uniform cross sections, however, and any haunching would affect the results. Values for the usual range of span lengths in two, three, and four spans are given, but any unusual span ratios would prevent the use of these tables. Furthermore, these tables, having been printed in Germany, are not available in the United States. The tables by Walter Ruppel, Assoc. M. Am. Soc. C. E., should also be mentioned.[36]

It would be interesting if the author would show how his method, applied to tall building frames, gives a fast and accurate solution to this difficult problem.

Once this method is mastered, it will be found to be a quick solution for most indeterminate stress analyses. The writer can set up a three-span or a four-span problem faster by the Cross method; but, for any problems of involved conditions, the Brumfield method is faster because the moment at the joint is only "transmitted once." For moving loads on a continuous span, this method is a "must."

[3] "The Solution of Statically Indeterminate Structures," by R. C. Brumfield, 1933 (printed in mimeograph form by The Cooper Union, New York, N. Y.).

[34] Cons. Engr., Roselle, N. J.

[34a] Received by the Secretary December 7, 1945.

[2] "Analysis of Continuous Frames by Distributing Fixed-End Moments," by Hardy Cross, *Transactions*, Am. Soc. C. E., Vol. 96 (1932), p. 1.

[35] "Zehnteilige Einflusslinien," by Anger, Ernst and Son, Berlin, 1939.

[36] *Transactions*, Vol. 90 (1927), p. 167.

AMERICAN SOCIETY OF CIVIL ENGINEERS

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DISCUSSIONS

DESIGN DEVELOPMENTS—STRUCTURES OF THE TENNESSEE VALLEY AUTHORITY

A SYMPOSIUM

Discussion

By William P. Creager

William P. Creager,[13] M. Am. Soc. C. E.[13a]—The painstaking care with which the authors and their colleagues have conducted research and tests on hydraulic structures is evident in these papers.

In Mr. Riegel's paper one test is missing—namely, that for the percentage of area of the base of dams subjected to uplift pressures. For several years, the Power Division of the Society has had a Subcommittee on Uplift in Masonry Dams, whose purpose was to devise experiments and report on its findings. The subcommittee has not reported, probably because it could devise no tests that would show conclusively whether only a part of the area or 100% of the area is subject to uplift. Notwithstanding the absence of such tests, the Tennessee Valley Authority (TVA) has assumed that only two thirds of the area is subjected to uplift pressure, after taking into consideration the maximum possible effect of the grouted cutoff and drainage system. With respect to this feature the writer presumes to take issue.

Uplift pressures under dams should be analyzed strictly according to two factors: (*a*) The hydraulic gradient factor and (*b*) the area-of-pressure factor.

(*a*) *Hydraulic Gradient Factor.*—Fig. 23 indicates a typical hydraulic gradient under a solid concrete dam. The piezometric pressures at the base vary from tailwater pressure h at the toe to tailwater pressure plus a percentage C of the head H on the dam at the drains and thence to full headwater pressure at the heel. The coefficient C is an indication of the effectiveness of the grouted cutoff and the drainage system; and w is the unit weight of water.

The hydraulic gradient factor, for the TVA main river dams, has been shown by Mr. Riegel to correspond to Fig. 23 with an adopted coefficient C equal to

Note.—This Symposium was published in October, 1945, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: December, 1945, by A. T. Larned.

[13] Cons. Engr., Buffalo, N. Y.

[13a] Received by the Secretary November 27, 1945.

zero. The writer has no criticism of the hydraulic gradient factor adopted for the main river dams, since subsequent tests have shown the adopted factor to be correct. Incidentally, it corresponds to 100% efficiency of the drains. However, attention is called to the fact that similar tests[14] on many other dams with grouted cutoffs indicate much less than 100% efficiency of drains. Either the nature of the rock or the excellence of the engineering has resulted in high efficiency of the TVA drains. It is doubtful if such high efficiency can be assured for all future similar dams.

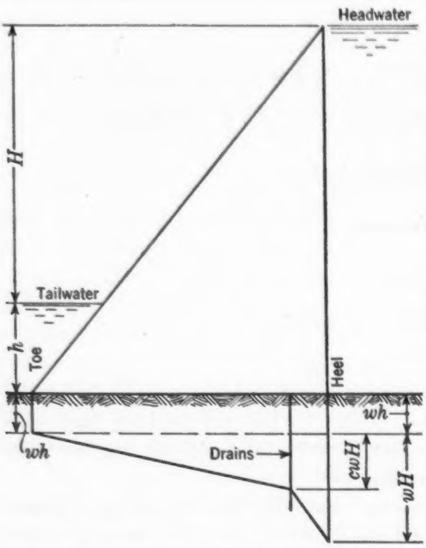

Fig. 23.—Typical Hydraulic Gradient

(*b*) *Area-of-Pressure Factor.*—The area-of-pressure factor for all TVA dams was assumed to be equal to two thirds of the area. The area-of-pressure factor has been the subject of much debate, ranging from about 50% to 100% of the area. The writer will not attempt to reopen arguments on this subject except to state that, by the very nature of so-called solid materials, if the area of pressure is less than 100%, the material must be capable of resisting tension.

Conversely, if the material, or any layer or bedding plane in the material, is of such a nature that it would not have appreciable tensile strength, then it should have an area-of-pressure factor of 100%.

The TVA main river dams were designed on the assumption that two thirds of the area of the base was subjected to uplift pressure. This area-of-pressure factor was adopted for "calcareous rocks with approximately horizontal bedding considered favorable to the accumulation of uplift pressures in the foundation itself." Thus, this factor is assumed to be advantageous in the accumulation of uplift pressures without a grout curtain and without drainage installation. Therefore, it is favorable to the accumulation of whatever uplift pressure remains after taking into account the full effect of the grout curtain and drainage on the hydraulic gradient.

For a river wall, supporting earth and with river depth and ground-water depth equal on each side of the wall, the use of less than 100% uplift would not be considered unless the foundation were capable of resisting tensile stresses; but this assumption is exactly what was made with respect to tailwater uplift in the TVA main river dams where the foundation is "favorable to the accumulation of uplift pressures."

The TVA criterion for uplift is not conservative when compared with what

[14] "Uplift Pressure in Masonry Dams," by Ivan E. Houk, *Civil Engineering*, September, 1932, p. 578.

the writer believes to be common practice, and the writer cautions against adopting it without the advance knowledge that the drainage system will be permanently 100% effective and that the rock is of such a nature that 100% uplift will not occur over the area of the base.

In the case of the higher dams, Mr. Riegel states that one half of the headwater pressure was assumed at the line of drains.

It might be mentioned that, as shown in Fig. 23, the pressure at the drains must be equal to tailwater pressure plus a percentage of the net head on the dam —because the effect of the grouted cutoff and the drains is not a function of the headwater pressure but is a function of the net head in the dam. Incidentally, Fig. 3 does not show one half of the headwater pressure at the drains, as Mr. Riegel states; but it does show the correct relation of tailwater pressure plus one half of the net head on the dam.

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DISCUSSIONS

DESIGN OF ROADBEDS

Discussion

BY RENÉ S. PULIDO Y MORALES

RENÉ S. PULIDO Y MORALES,[35] ESQ.[35a]—Soil is the most important of the infinite varieties of material in a roadbed. For this reason and may others, engineers are developing the new science of soil mechanics and pedology in order to explore the properties of the different constituents of soils. Mr. Mullis' work is another valuable contribution to the science of pedology.

In the middle of the nineteenth century research investigators, in the fields of physics and chemistry, first approached a study, and thus gained an understanding, of the properties of gases and liquids in the soil, and of their influence on mechanical structures. In 1930, N. F. Lebedeff stated[36] that water occurs in soil as:

(1) Vapor water in the voids, fissures, underground channels, and cavities;

(2) Hygroscopic water absorbed by soil particles from the vapor of the air (according to R. Rodewald[37] this water forms a monomolecular film on the particles;

(3) Film water, that covers the soil particles and is retained by gravitational molecular forces (this process is a continuation of the hygroscopic absorption in item 2);

(4) Gravity water, further subdivided as—

- (*a*) Capillary water that ascends from the phreatic water,
- (*b*) Suspended water that fills the pores of the soil but without communication with the phreatic water, and
- (*c*) Gravity water or free water that flows downward by gravity force;

(5) Solid water;

(6) Crystallized water; and

(7) Water containing chemicals.

NOTE.—This paper by Ira B. Mullis was published in April, 1945, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: October, 1945, by Charles W. Britzius, J. T. L. McNew, and Gustavo Pérez Guerra; and November, 1945, by Jacob Feld, and Jasper L. Stuckey; and December, 1945, by Delbert L. Lacey.

[35] Civ. Engr., Havana, Cuba.

[35a] Received by the Secretary November 21, 1945.

[36] "Soil Water," by N. F. Lebedeff, Moscow, 1930.

[37] "Uber Quellungs und Benetzungserscheinungen," by R. Rodewald, 1900.

The complexity of the system, soil-water-air, is apparent from the many forms that water can assume. This complexity and the variety of forms, sizes, and types of soil particles explain why no theory is valid for all soils, and why a liberal engineering judgment is necessary to produce a successful and stable road design.

In all soil mixtures the maximum stability is obtained at the highest density. The free water acts like a lubricant for the individual particles. The density varies according to four moisture phases; that is: (*a*) Hydration,[38] (*b*) lubrication,[39] (*c*) excess water and air, and (*d*) total saturation.

The optimum moisture occurs in phase (*b*); in phase (*c*) the volumes of free water and free air exceed the volumes of the pores, and the density diminishes because the specific gravity of the water is less than the soil particles; and, in phase (*d*), the water begins to occupy the air space, thus completing the total saturation of the soil.

In soils with a great quantity of large particles, the maximum density is obtained easily; probably because the large particles form a structure that permits transmitting the exterior forces deeper into the subsoil. M. M. Filatoff[40] states that the optimum grading of materials to secure stability in a roadbed soil is obtained by studying their cohesive qualities. His experience indicates that stability is dependent on the clay fraction although it is evident that under different conditions this clay fraction can possess different properties. The maximum and minimum hygroscopicity for clay soils is sometimes very useful. When the maximum is greater than the plastic limit, the soil is not suitable for flexible pavements. Factors such as these, that affect the capacity of a soil to support external forces, are very numerous.

The complexity of the system, soil-water-air, is the cause of the many failures in roadway design. The subject, so well presented by Mr. Mullis, deserves coordinated and widespread research by everyone interested in it.

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[38] "Essentials of Soil Compaction," by C. A. Hogentogler, *Proceedings*, Highway Research Board, National Research Council, Vol. 16, 1936, p. 309.

[39] "Fundamental Principles of Soil Compaction," *Engineering News-Record*, August 31, 1933, p. 245; September 21, 1933, p. 348; September 28, 1933, p. 372.

[40] "Principles of Pedology Applied to Road Construction," by M. M. Filatoff, Moscow, 1936.

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DISCUSSIONS

EFFECTS OF RADIANT HEAT ON REINFORCED-CONCRETE RIGID FRAMES

Discussion

BY A. A. EREMIN

A. A. EREMIN,[18] ASSOC. M. AM. SOC. C. E.[18a]—That radiant heat has an important effect on the stresses in bridge frames, as stated by Mr. Johnston, is undeniable. However, the writer does not agree with conclusion 3, namely, that the radiant-heat effect on the maximum moment at the crown may be obtained by considering "a substantial seasonal temperature drop (45° F or more from a mean value)." The action of radiant heat is not the same as that of seasonal temperature variation. The primary effect of radiant heat is to rotate plane sections whereas the effect of seasonal temperature variation is to displace joints. Furthermore, the author's computation of the stresses resulting from radiant heat may be simplified so as to make unnecessary the substitution proposed by the author.

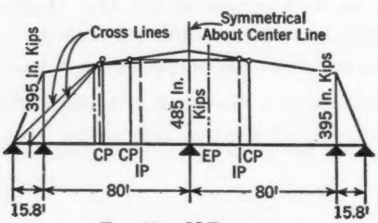

FIG. 10.—*M*-DIAGRAM

Radiant heat may be assumed to decrease uniformly along the slab section as shown in Fig. 5(*a*). Then, in the author's numerical example, the identical angular rotation of slab ($\phi = 2 \times 10^{-6}$ radians) may be obtained from Figs. 4 and 5(*a*) by assuming $t_f = 16.5°$. The end rotations of a beam may be expressed as

$$\theta_A = \theta_B = (e + e_0)\frac{L}{I} \qquad (18)$$

in which the constants e and e_0 vary with the shape of member and are read

NOTE.—This paper by Milan A. Johnston was published in June, 1945, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: October, 1945, by I. Oesterblom; and November, 1945, by N. M. Newmark, and L. C. Hollister.

[18] Associate Bridge Engr., Bridge Dept., Div. of Highways, State Dept. of Public Works, Sacramento, Calif.

[18a] Received by the Secretary November 19, 1945.

from prepared diagrams.[19] For the slab in Fig. 2, $e = 0.168$ and $e_0 = 0.112$. Substituting the values of constants in Eq. 18, $\theta_A = \theta_B = 10{,}300$, which is very close to the value computed by the author.

Fixed-end moments induced by radiant heat may be distributed graphically by the use of "cross lines," as shown in Fig. 10. From the foregoing it is evident that the effect of radiant heat can be computed very quickly without the analogy proposed by the author. The best result is always obtained by a direct estimate of the cause.

[19] "Analysis of Continuous Frames by Graphical Distribution of Moments," by A. A. Eremin, Sacramento, Calif., 1943.

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DISCUSSIONS

DETERIORATION OF CONCRETE DAMS DUE TO ALKALI-AGGREGATE REACTION

Discussion

By Duff A. Abrams

Duff A. Abrams,[11] M. Am. Soc. C. E.[11a]—A notable feature of the paper by Messrs. Blanks and Meissner is its many contradictory statements. A few of the more obvious contradictions are listed here as an introduction to the arguments to follow:

Cracks in Boulder Dam (see heading, "Progressive Development of Alkali-Aggregate Deterioration in Concrete Dams") "must be ascribed to extreme drying exposure in the hot, arid location," but in Parker Dam, made of the same cements, a similar concrete mix, built by the same contractor, using the same equipment, in a hotter and more arid location, the cracks were ascribed (see heading, "Significant Effects of Alkali-Aggregate Deterioration on the Service Life of Concrete Dams and Appurtenant Works") to alkali-aggregate reaction. Surface cracks in concrete dams are attributed to internal expansion due to alkali-aggregate reaction, but the photographs that illustrate this phenomenon clearly show contraction cracks.

The mortar-bar expansion test on sand from Owyhee Dam shows the highest expansion of any sand reported. Photographs of "affected areas" (Fig. 8) do not show Owyhee Dam, but only part of a small viaduct.

Nearly every positive assertion by the authors is modified or nullified by contrary statements elsewhere in the paper.

Failure of Parker Dam.—A number of concrete dams were mentioned; but, to the writer, the paper seems primarily an attempt to explain the failure of Parker Dam on the Colorado River 155 miles south of Boulder Dam. This is an arch dam, 322 ft high, 100 ft thick at the base, and 39 ft thick at the roadway level, containing 276,000 cu yd of concrete. It was completed in 1938 by the U.S. Bureau of Reclamation (U.S.B.R.) for the Metropolitan Water District of Southern California, at a cost of $9,000,000. Two years later it was found to be in an advanced stage of disintegration.

Note.—This paper by R. F. Blanks and H. S. Meissner was published in January, 1945, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: September, 1945, by Ralph R. Proctor, Milton D. Burris, H. A. Kammer, Thomas E. Stanton, R. W. Spencer, and W. L. Chadwick; and December, 1945, by J. C. Witt.

[11] Cons. Engr., New York, N. Y.

[11a] Received by the Secretary September 4, 1945.

It may be thought that "failure" is not the correct term, since, at last report, the dam was still in service. The following statements were made by the chief engineer, U.S.B.R., early in 1941 (16)[11b]:

> "In the construction of Parker Dam full advantage was taken of every known advancement and modern improvement in the selection and utilization of materials, production control, construction methods and equipment; the concrete was cooled and no precaution was overlooked to insure the very highest quality of construction in that dam. Nevertheless, we have a dam there whose permanence must be very seriously questioned.***
>
> "Of course, Parker Dam is not going to fall down next year or the year after, but it is going to require a lot of maintenance work, and there is the possibility that at some time in the distant future it may have to be largely replaced."

When the chief engineer, two and one-quarter years after completion, sees a large concrete dam "whose premanence must be seriously questioned," and a dam that "may have to be largely replaced," the only accurate term is "failure." After five years of "intensive investigation," the authors attributed the failure to reactions between the alkalies in the cement and the aggregates.

This discussion examines the authors' evidences of alkali-aggregate reaction in concrete. It will be shown that the failure of Parker Dam was due to an entirely different cause, and that it was exactly what should have been expected from properties and behavior of concrete that had long been well known to the profession.

High-Alkali Cements.—It has been known for nearly a century that Portland cements contain alkalies. The first complete analysis of Portland cement was reported, in 1849, by A. Hopfgartner (17), an Austrian chemist, from tests made on English cement that gave 2.7% alkalies. Every subsequent complete analysis of Portland cement has shown alkalies. It is doubtful whether a commercial cement was ever made that did not contain alkalies.

Prior to the construction of Boulder Dam, the U.S.B.R. spent a large sum of money in studies of cement and concrete (18)(19). As a result of these studies they prepared a specification for what is now known as a low-heat cement. The principal characteristic of this cement is the high ratio of dicalcium silicate. The most direct method of producing such a cement is to reduce kiln temperature. It has long been known that low kiln temperatures increase the alkalies in the cement. This was brought to the attention of American engineers by W. F. Hillebrand (20) in 1903, and by A. V. Bleininger (21) in 1904.

The low-heat cements used in Parker Dam were manufactured by the same four mills that furnished cement to Boulder Dam.

"Gel Deposits."—The incomplete chemical analyses in Table 1 are the only data that have been released on the composition of the "gel deposits" from cores taken from Parker Dam. These analyses were first made public in February, 1941 (6). The entire case of "gel deposits" collapses in the light of evidence that the cores were packed in damp sawdust for shipment to Denver,

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[11b] Numerals in parentheses, thus: (15), refer to corresponding items in the Bibliography (see Appendix of the paper), and at the end of discussion in this issue.

Colo. (22). Many woods contain organic acids that readily attack compounds in cements and aggregates. Evidences of acid action would be destroyed by exposing the samples to a high temperature. The nature of the material that was driven off in the "loss-at-ignition" test (Table 1) was not disclosed; nor was the temperature given. These losses seem to represent free water, although it may have been carbon dioxide, water of crystallization, alkalies, or many other compounds.

The material that the authors identified as water glass was probably formed in the crucible during the "loss-at-ignition" test. Water glass is formed only under the most favorable conditions and at a temperature high enough to fuse alkalies and silica; it certainly was not formed in this concrete.

Mortar-Bar Tests.—The writer made a serious effort to ascertain what the facts were with reference to the mortar-bar expansion tests reported by the authors, but was soon lost in conflicting tests and in "yes-no-maybe" language. Typical statements were:

Statement A (See Heading, "Summary").—

> "Aggregate which will give this trouble in concrete may be recognized if it produces expansion within a short time in test specimens made with high-alkali cement."

Statement B (Last Paragraph Under "Symptoms and Diagnosis of Alkali-Aggregate Deterioration in Concrete Dams").—

> "The test is useful only as an indication of possible troublesome combinations of reactive aggregates and high-alkali cements."

Statement A is positive and unequivocal, but it is completely nullified by Statement B. The key words in Statement B are "indication" and "possible." Used in science or engineering, "indication" is a "strong" word; Webster's Dictionary gives the synonyms "manifestation, mark, evidence, signal, and proof." The word "possible" is "weak" and takes a position at the opposite end of the scale. Something that is "useful only as an indication" of a possibility is no indication at all. The net result of the two statements is a sort of double vacuum.

Another example is afforded by the sentences preceding Statement B:

Statement C.—

> "Although excellent correlations have been obtained between laboratory results and service performance, the mortar-bar expansion test has not been developed so that it can be relied upon as an infallible indication of troublesome combinations of cements and aggregates. Neither can it be considered as a satisfactory acceptance test for either cements or aggregates."

What were these "excellent correlations"? In no instance was the cement used in the tests, the same as that used in the dams which showed "deterioration due to alkali-aggregate reaction." In the case of "Crushed Phyllite, Buck Dam in Virginia" (Fig. 7), the test showed a very rapid expansion; in three months it was 0.4%, but, for the dam, expansion was not observed until the concrete was ten years old, and it required eighteen years to reach 0.5% (2).

"Excellent correlations" would of themselves establish the test as an "infallible indication of troublesome combinations" of cement and aggregates. The positive assertion ahead of the comma in Statement C was promptly canceled by the contrary statement after the comma.

Statements A, B, and C are fair examples of what the authors have to say on the mortar-bar expansion test after using it for five years; but the tests in Fig. 7 cannot be accepted as evidence of alkali-aggregate reaction. A few of the reasons are: (*a*) The reader is not informed as to the composition or any other properties of the sands; (*b*) he is not told the alkali content or any other properties of the cements; and (*c*) he is not informed whether all high-alkali tests were made with the same cement. Perhaps the excessive expansion found with certain sands and high-alkali cement was due to high free lime that generally accompanies low burning temperatures. Furthermore: (*d*) With high-alkali cement, crushed andesite from Friant Dam showed, at three months, seven times the expansion of Parker sand, but in an earlier report Mr. Blanks (4*d*) showed that Parker andesite gave no appreciable expansion at three months; (e) at six months, with high-alkali cement, Owyhee sand showed eight times as much expansion as Parker sand, but there was no evidence to show that Owyhee Dam expanded several times as much as Parker Dam; and (*f*) no values were given for Owyhee and American Falls sands with low-alkali cement. Perhaps these tests showed more expansion than the high-alkali cement.

The writer finds no foundation for the claim of "excellent correlation" between the mortar-bar test and service performance. The mortar-bar tests are inconclusive and misleading, and bear no relation to what occurs in a dam. The conflicting treatment of the paper supports the writer's deductions.

Reactive Aggregates.—The entire burden of the paper is that alkali-aggregate reaction was the cause of the deterioration of concrete dams; but, when the reader inquires into the scientific basis for this hypothesis, and attempts to identify the "reactive aggregates," he encounters insurmountable dificulties.

Opaline Silica.—This material heads the list of rocks that "have been found to react with high-alkali cement." What is "opaline silica"? It is not defined by any dictionary in the New York Public Library; it does not appear in the index of the Encyclopædia Britannica; and it is not found in the index of Chemical Abstracts, started in 1907. A commercial house that has supplied mineralogical specimens from all parts of the world for eighty-two years could not fill an order for a sample of "opaline silica." Dana's "Manual of Minerology" has been a standard reference work for nearly one hundred years; "opaline silica" did not appear in any of the fifteen editions of this book published between 1848 and 1941. Opal is a form of hydrous silica; hence "opaline silica" is a redundancy which being translated becomes: "(hydrous-silica)—like silica."

Rhyolite.—"Some rhyolites" were found by the authors among the "more conspicuously reactive" aggregates; but a rhyolite (pumicite) was used to replace 20% of the cement in Friant Dam, and the authors "felt that pumicite has furnished some protection against expansion where the low-alkali cement was not provided" (see heading, "Corrective and Preventive Measures"). In other words, "some rhyolites" are reactive with high-alkali cement; but, when

the rhyolite is very finely divided, contains 9% alkalies, and is called "pumicite," it is not harmful, but has distinct merits when used with a high-alkali cement.

Andesite.—The writer was struck by the use of the word "some," which occurred seven times as a modifier of various rock types that the authors claim to be reactive with high-alkali cement. Fortunately, there is evidence by which to determine what was included in "some andesites." Early papers by the authors (6)(22)(4d) claimed that andesite was the principal reactive aggregate in Parker sand. In 1941, however, Mr Blanks (4d) published the results of tests on mortar bars with a high-alkali cement and 10% of Parker andesite crushed to four different finenesses. All tests showed negligible expansion at one, two, and three months. The authors now give, in Fig. 7, a curve for "crushed andesite, Friant Dam," which shows a very high expansion. The native rock at Boulder Dam is described as "andesite breccia," but, according to the authors (see heading, "Progressive Development of Alkali-Aggregate Deterioration in Concrete Dams"):

Statement D.—

"The only place where alkali-aggregate reaction had been definitely identified at Boulder Dam, after eight years of service, was in the lining of a drainage tunnel through some wet rock."

Obviously, this rules out Boulder Dam andesite, although the same high-alkali cements were used as at Parker Dam. In two out of three instances andesite has been found nonreactive. This reduces "some" to less than one third. It is more logical to conclude that andesite is nonreactive, and that repeat tests on the same material gave conflicting results.

Other Rock Types.—In view of what has been written herein under "andesite," little weight can be given to the authors' claims (see heading, "Symptoms and Diagnosis of Alkali-Aggregate Deterioration in Concrete Dams") that:

Statement E.—

"Some of the more conspicuously reactive aggregate materials include: Opaline and other siliceous rocks such as some cherts, some siliceous limestones, some chalcedonies and intimate opal-chalcedony admixtures; certain acid to intermediate volcanic rocks such as some andesites and rhyolites, including tuffs and glasses of similar composition; and miscellaneous rock types such as phyllite."

The authors' term "other siliceous rocks" (a meaningless classification in regions where all rocks are siliceous) just about exhausts the rock types that are claimed to be reactive. The only conclusion that the writer can draw from the data and the statements of the paper is that the authors failed to name a single recognizable rock type that is reactive with high-alkali cement.

Alkali-Silica Reaction.—The paper encourages readers to believe that an alkali-silica reaction had been identified as the cause of the "alkali-aggregate disease in concrete dams," but the expression generally used was "alkali-aggregate reaction." The following is typical of the misleading inferences of the paper (see heading, "Symptoms and Diagnosis of Alkali-Aggregate Deterioration in Concrete Dams"):

Statement F.—

> "Far too little is known about the specific mineral constituents of various rock types which contain silica in forms susceptible to reactivity with alkalies in cement to permit positive predetermination of troublesome combinations by petrographic analyses."

To make such a statement regarding "silica in forms susceptible to reactivity with alkalies" begs the question; it merely asserts something that has not been proved. Dozens of scientists have devoted their lives to the study of silica. The properties of the various forms of silica that occur in nature are fairly well understood. There is no evidence to show that the authors made any attempt to determine whether or not these forms are reactive with alkalies. Alkalies are among the most widely distributed compounds in nature. The fact that silica in sand and gravel has not been destroyed by alkalies is of itself strong evidence that it is not reactive.

The only parts of the paper that bear on this question are the high silica content of one of the "gel deposits" in Table 1, and the statement that "The gels may be transparent, closely resembling ordinary water glass***." The writer has shown that: (*a*) No credence can be given to the inexpert and incomplete chemical analyses of the "gels"; (*b*) water glass cannot possibly be formed in the concrete; and (*c*) not a single recognizable rock type was identified as reactive with high-alkali cements. In other works, the paper contains no evidence of alkali-silica reaction.

Before the hypothesis of alkali-silica reaction becomes acceptable, the authors must explain:

(1) If certain constituents of Parker sand were attacked by cement alkalies in the dam, why were they not long ago destroyed by the same alkalies in Williams River water (a tributary to the Colorado River)?

(2) If 5 lb of cement alkalies is a menace to a cubic yard of concrete, why was the aggregate not long ago destroyed by 225 lb of the same alkalies that they contain?

(3) Parker Dam cements contained 16 to 47 times as much silica as alkalies. Why did not this silica combine with the alkalies during hydration of the cement?

(4) Parker Dam cement had 1.2% alkalies; the mortar contained 0.3% of cement alkalies. The "gel deposits" had 17% alkalies, or 55 times as much as the mortar. How can the cement alkalies move about so freely within the hardened concrete? Why did this high concentration of alkalies occur in the hardened concrete and not in the cement paste alone, or in the aggregate alone?

The mere statement of the foregoing considerations discredits the hypothesis of alkali-silica reaction in concrete. In the case of Parker Dam, the authors gave this hypothesis a slight semblance of credibility by: (*a*) Incomplete, inconsequential tests, (*b*) foggy language, and (*c*) playing down or failing to present important information on concrete materials and test methods.

Solution of an Enigma.—In the foregoing paragraphs the writer has shown that: (*a*) The mere assertion that there are "rock types which contain silica in

forms susceptible to reactivity with alkalies in cement," does not constitute a demonstration; (*b*) incomplete chemical analyses cannot be accepted as a basis of a hypothesis of alkali-silica reaction; (*c*) contradictory statements on the results of mortar-bar tests make those tests valueless; and (*d*) such expansion as was found was probably due to causes other than alkali-aggregate reaction. The remainder of this discussion is constructive in that it solves the "enigma" of the failure of Parker Dam.

Parker Dam Climate.—The school geography used by the writer had printed across the map of the southwestern part of the United States: "The Great American Desert." At Parker Dam the sun shines nearly 100% of the possible hours, shade temperatures reach 125° F, and concrete surface temperatures 145° F. The air is almost devoid of moisture; relative humidities of 6%, 5%, and 4% are frequently recorded. In parts of this desert 10-yr periods have passed without any measureable rainfall. It is doubtful whether a large concrete structure has been built any place in the world where more severe combinations of high temperature and low humidity were encountered. The U.S.B.R. has used the terms "blistering desert," and "almost unbearable heat," to describe the conditions at Boulder Dam, which is 155 miles north of Parker Dam.

Concreting Parker Dam.—In many important features of the design and construction of Parker Dam, there was failure to take account of the severe climate, and well-known properties and behavior of concrete were ignored. Three examples are given:

(1) Grinding of Aggregates in Mixer.—Concrete was mixed in 4-yd batches. The specifications required concrete to be mixed 2½ min. Since 1870, leading authorities have repeatedly warned against overmixing to such an extent as to produce excessive breaking or grinding of aggregates. Three important series ot tests were conducted before concreting was begun on Parker Dam in 1937. The late W. A. Slater, M. Am. Soc. C. E. (23), made such tests in 1930; S. C. Hollister, M. Am. Soc. C. E. (24) in 1931; and W. A. Blanchette (25) in 1934. All studies showed that long mixing, using gravel up to 1¼ in., resulted in grinding large quantities of aggregates to the fineness of cement. Little argument will be required to show that even with modest times of mixing, excessive grinding occurs with gravel up to the size of a football. An ideal grinding machine is created by 17,000 lb of water, cement, sand, and gravel as large as 6 in. The writer has shown (26) that at Grand Coulee Dam (on the Columbia River, Washington), 4-yd mixers produced rock dust equivalent to 26% of the weight of the cement in 2½ min, and 95% in 15 min. There are several reasons for the belief that there was more mixer grinding at Parker Dam than at Grand Coulee Dam.

(2) Rate of Concreting.—The rate of placing concrete should be restricted to permit the dissipation of part of the heat of hydration of the cement. Parker Dam Specifications provided (27):

> "The rate of placing concrete in any panel or block of the dam shall be such that not more than 5 ft. shall be placed in 72 hours, and not more than 30 ft. in depth shall be placed in 30 days unless specifically authorized by the contracting officer."

Construction records show that, in the hottest months of the year, neither of the foregoing provisions was enforced. In many instances 10 ft or 15 ft of concrete was placed in 72 hours. Instead of limiting the depth to 30 ft in 30 days, 45 ft to 70 ft was placed in a month. In three instances, 60 ft was placed in a month; in seven instances, 65 ft; and in six instances, 70 ft.

(3) Curing Concrete.—The importance of adequate curing of concrete has been well understood since 1920. How was the challenge of this "blistering desert" met at Parker Dam? The Specifications required (27):

> "For concrete made with low-heat or moderate heat cement the period of moist curing shall be extended to not less than three weeks to correspond to the minimum curing period of two weeks for concrete made with standard portland cement. The method of keeping the concrete moist shall be by continuous sprinkling, spraying, or other methods approved by the contracting officer."

A concession of one week (which was not enforced) was made on account of the low-heat cement, but no concession whatever was made on account of sun temperatures up to 145° F or the near-zero humidity. The upper, thin, reinforced concrete sections of the dam were not cured by "continuous sprinkling, or spraying," but by three coats of a patented curing compound that was poorly adapted to the conditions.

Cracking of Parker Dam.—On this job every effort should have been made to restrict mixing water to a minimum. By a better grading of aggregates, and a method of mixing that minimized mixer grinding, the mixing water in a cubic yard of concrete could have been reduced from 250 lb to 175 lb. All concrete should have been effectively water cured for the entire summer after the main portion of the dam was finished early in 1938. Instead, the concrete contained a great excess of mixing water, and the curing was not up to the standard practice in regions with a favorable climate. Surface cracking at Parker Dam was due to too much mixing water and to inadequate curing. The effects of these factors were well understood long before this dam was begun. It is not necessary to search for a mysterious alkali-aggregate reaction to explain these cracks.

The first twelve pages of the paper give the impression that serious deterioration was shown by the internal expansion that caused surface cracks in concrete dams; in the remainder of the paper the authors' attempt to demonstrate that such cracks "would appear to be of minor importance." The work "crack" (cracks, cracking, crazing) was used about forty times, but the authors concentrated the reader's attention on surface "random-pattern cracking." Not once did they refer to structural cracks in either the dam or the superstructure. The structural cracks contradict the alkali-aggregate hypotheses of the paper.

Expansion Measurements at Parker Dam.—To the writer, the curves in Fig. 10, presented to "indicate progressive expansion in the concrete of Parker Dam," have a meaning entirely different from that suggested by the paper. There is a well-known cause of concrete expansion that has nothing to do with alkali-aggregate reaction, which certainly was present in this dam—namely, temperature rise due to the hydration of the cement. This effect of the heat of hydration has long been recognized, and has generally been partly counter-

acted by an embedded-pipe system of cooling the concrete. Cooling was not applied to the abutments, however, and it is probable that the cooling of the other portions of the dam was ineffective, since the cooling medium was river water and cooling was delayed until serious damage had occurred.

The trend of the curves in Fig. 10 shows a net expansion, in the 5-ft to 10-ft range, of about 0.00013 per yr. Assuming a coefficient of thermal expansion of 0.0000055, this expansion would be accomplished by a temperature rise of 24° F per yr. If this continued for five years, from the time the concrete was placed until the end of 1942, it means a total rise of 120° F.

Parker Dam has five 50-ft by 50-ft gates. The two abutments, outside of the gate system, are about 375 ft long. The total horizontal movement into the gate openings in five years would then be: $0.00013 \times 5 \times 375 \times 12 = 2.9$ in. In the same way there would be a "hump" of about $2\frac{1}{2}$ in. in the middle of the dam due to vertical expansion. This placed the gates, the gate-operating structure, and the roadway bridges in a gigantic vise where they were crushed like eggshells. It is not surprising that there was considerable "binding of gates" which the authors mention three times.

The expansion, in which the authors (see heading, "Symptoms and Diagnosis of Alkali-Aggregate Deterioration in Concrete Dams") mistakenly saw "signs of abnormal expansive movements in concrete which accompany alkali-aggregate reactions," was due to hydration of cement. The measured expansion (or computed temperature rise) enables one to estimate the excess stress developed in the restrained concrete in the dam proper. Assuming a modulus of elasticity of 4,500,000 lb per sq in., the stress would be: $0.00013 \times 5 \times 4{,}500{,}000 = 3{,}250$ lb per sq in. Part of this excess stress near the top of the dam was relieved, no doubt, by pushing the crown of the arch upstream. In an earlier discussion (28) the writer gave some evidences of similar high stresses in Boulder Dam, due to unconsidered temperature rise, as a result of continuing hydration of cement.

That the writer's analysis is not mere theory is shown by two other facts: (*a*) During construction, a wedge of concrete about 100 ft long broke off where the downstream corner of one of the abutments joined the rock; (*b*) the reinforced concrete girders that carry the highway over the 50-ft gate openings showed, within a few months after completion, that they had been squeezed and crushed like so many eggshells. The foregoing phenomena were clearly not due to alkali-aggregate reaction; they can be accounted for readily by temperature rise of the concrete; they were not mentioned by the authors.

Missing Data.—A large number of extensometers and thermometers were embedded in Parker Dam. Instead of giving these data which have been available from the beginning, the authors base their conclusions on expansion and temperatures as measured in four shallow holes that were drilled in an abutment several months after the dam was known to be in a precarious condition. The deepest of these holes was 3% of the maximum depth of the dam.

It seems obvious that the "concrete temperature" curve at the top of Fig. 10 does not represent the interior of the dam; the dam temperature could not possibly show seasonal variations of 40° F. A study of the curves suggests that the temperatures for the 30-in. hole were plotted; the points do not, in general,

coincide with the dates of expansion readings in the 12-in., 60-in., or 120-in. holes.

The authors are requested to give for Parker Dam: (*a*) Concrete temperatures for, say, April, 1938, 1939, 1940, 1942, and 1945—in an abutment at mid-depth of the gate opening and in the middle of the dam 50 ft below tailwater level; (*b*) the horizontal movement of each abutment into the gate opening; (*c*) the rise of the top of the dam; (*d*) the upstream movement of the crown of the arch; and (*e*) all temperature readings taken in the four holes drilled in an abutment.

Conclusions.—This discussion is a condensation of a more voluminous study of the paper. The space available makes it necessary to omit many significant features, and merely to state some of the conclusions without supporting data or arguments.

General.—

(1) The paper was notable for its many contradictory statements; practically every positive assertion of the authors was modified or nullified by contradictory statements elsewhere in the paper.

(2) To the writer it appears that the authors did not avail themselves of evidence that contradicts the hypothesis of the alkali-aggregate reaction.

(3) The photographs that were given to illustrate expansive cracking of dams clearly show cracks to be due to contraction of concrete.

(4) The only general conclusion that can be drawn logically from the paper is that dams should not be built of concrete. The writer does not subscribe to that conclusion.

Alkali-Aggregate Reaction.—

(5) The "gel deposits" from Parker Dam concrete probably resulted from attack by organic acids; this accounts also for the "chalky, lifeless appearance" of the concrete. These phenomena were not due to alkali-aggregate reaction.

(6) The resemblance of the "gel deposits" to water glass probably resulted from a manufacture of a glass in the crucible at the high temperature of the ignition test. It is certain that water glass was not formed in the concrete. There was no evidence to show that the "gel deposits" were harmful to concrete.

(7) Five years of "intensive investigation" failed to identify a single rock type that reacted with high-alkali Portland cement. The hypothesis of alkali-silica reaction has no basis except foggy language, unsupported assertions, and trivial circumstances.

(8) The "yes-no-maybe" language of the paper makes it impossible to determine what the authors' conclusions were with reference to the mortar-bar expansion tests. The tests that have been reported are contradictory and misleading.

(9) The laboratory tests were in general meaningless, due to limited scope, lack of scientific controls, and faulty technique. In most instances the material tested did not remotely resemble the concrete used in dams.

(10) The authors' blanket indictment of all types of concrete aggregate "with the possible exception of pure limestone" (that does not occur in Nature)

is not supported by data in the paper; it is contradicted by one hundred years of experience by the concrete industry.

Failure of Parker Dam.—

(11) Since January, 1940, it has been fashionable to attribute concrete failures to alkali-aggregate reactions. The evidence shows that Parker Dam was in a precarious condition early in 1940, less than two years after completion. Failure was at once attributed by U.S.B.R. to an excess of 0.6% of alkalies in the cement that they had designed especially for this work.

(12) Concrete specifications were inadequate for this "blistering desert" climate; the few ineffective requirements were ignored.

(13) Abnormal surface cracking was due to excessive volume shrinkage of concrete as a result of rapid evaporation of mixing water. The excessive quantity of mixing water resulted from poorly graded and contaminated aggregates and from excessive mixer grinding. Surface cracking could have been reduced to negligible proportions by exercise of well-recognized precautions with reference to materials, proportions, and curing.

(14) The damage to Parker Dam is much more serious, and from a cause entirely different from that suggested by the paper. Unconsidered temperature rise due to hydration of the cement resulted in a destructive expansion that the authors mistakenly attributed to alkali-aggregate reaction. This placed five large gates, the gate-operating structure, and the roadway bridges, in a gigantic vise, where they were crushed like eggshells.

(15) Too rapid placing of concrete, although prohibited by the Specifications, contributed to this destructive expansion.

(16) There is no evidence to show that high-alkali cement contributed in any way to the cracking, expansion, or failure of the dam. There is no reason to think the results would have been different if a low-alkali cement had been used.

(17) The measured expansion, given in the paper, corresponds to a computed compressive stress in the concrete of 3,250 lb per sq in. in excess of that for which the dam was designed.

(18) The trouble at Parker Dam was inherent in the proportions of concrete mixes and the concreting methods. If information that had long been available to the profession had been used, there is every reason to believe that this dam would be in perfect condition today—a constant reminder that (as stated by the U.S.B.R. in 1942) concrete "is in reality a simple and practical art."

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(25) **"Effect of Mixing Time on Quality of Concrete Produced in Large Mixers,"** by William A. Blanchette, *Public Roads*, Vol. 15, No. 9, November, 1934, p. 217.

(26) *Proceedings*, A.S.T.M., 1943, p. 1029.

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DISCUSSIONS

A PLAN FOR A MOVABLE DAM

Discussion

By Isaac De Young

Isaac DeYoung,[11] M. Am. Soc. C. E.[11a]—In Mr. Sabin's discussion, a historical review is given of the developments of the various forms of safety appliances adopted at the Sault canals to stop the free flow of water, in the event a lock gate is rammed by a vessel or otherwise damaged. Seven different devices are enumerated beginning with the caisson gate for the lock of 1855 to the present form of dam of the box girder type for the MacArthur Lock, completed in 1943.

The plan of the proposed dam is based on the latest conditions existing, in that the lock has a depth of 30 ft. The application of the form of dam will be more favorable to less severe conditions.

Since the first canal and lock were opened to navigation, at St. Marys Falls Canal, Michigan, in 1855, no accident has occurred that resulted in the urgent need for a movable dam. At various times, however, the gates of the locks at St. Marys Falls Canal have been struck by vessels, with potentially serious consequences, while maneuvering through the locks (see Table 1). An accident occurred at the Sault Ste. Marie Canal, Ontario, on June 9, 1909, in which all the gates, except the intermediate and guard gates, were wrecked, making the canal and lock an open raceway. A movable dam, of the swing-bridge type, was available to stop the free flow.

There have been no collisions with the gates of the Davis and Sabin locks since they were placed in commission in 1914 and 1919, respectively. The wooden gates of the Weitzel Lock, built in 1881, have been struck at various times by sailing vessels, but not to the extent that would require an emergency dam. More detailed information of the accidents may be obtained by reference to the Reports of the Chief of Engineers, U. S. Army.

All the gates of the locks at St. Marys Falls Canal are mitering gates. The Poe Lock, in addition to the upper and lower guard gates (which are used only

Note.—This paper by Isaac DeYoung was published in February, 1945, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: May, 1945, by C. L. Hall, C. E. Meyerdick, and Ralph R. Leffler; and June, 1945, by Jay L. Southworth; and October, 1945, by L. C. Sabin, and A. W. Sargent.

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[11a] Received by the Secretary November 16, 1945.

for unwatering the lock), has one pair of operating gates at the upper end of the lock, and two pairs at the lower end. All the other locks have two pairs of operating gates at both ends of the lock. In the lock of the Sault Ste. Marie Canal, Ontario (item 8, Table 1), there is one pair of operating gates at the upper end of the lock and two pairs of operating gates at the lower end.

TABLE 1.—REPORTS OF VESSELS COLLIDING WITH LOCK GATES

Accident No.[a]	Date	Name of steamer	Bound	Report, Chief of Engineers	
				Year	Page
1	August 4, 1899	*Sir William Siemens*	Up	1900	3961
2	June 3, 1901	*William R. Linn*	Up	1901	3173 3182
3	October 7, 1901	*Robert Fulton*	Down	1902	2225
4	November 10, 1909	*Isaac L. Elwood*	Down	1910	2231
5	August 3, 1910	*Lake Shore*	Up	1911	2424
6	September 13, 1912	*U. S. Steamer Essex*	Up	1913	2877
7	October 22, 1928	*Cambria*	Up	....	....
8	June 9, 1909	*Perry G. Walker*	Up	1909	2069

[a] Each collision occurred in the Poe Lock (St. Marys Falls Canal) except No. 8 which occurred at the Sault Ste. Marie Canal in Ontario, Canada.

In the accidents listed in Table 1, items 2, 4, 5, and 8 were the most serious. In accident 1, the upbound vessel struck the south leaf of the upper operating gate. Upon examination, after pumping out the lock, it was found that the gate was deformed only enough to prevent the use of the gate during the remainder of the season. Repairs were made during the following winter.

In accident 2, the vessel was approaching the lock upbound; it struck the downstream side of the lower south leaf, while the water in the lock was at the upper level and while upper gates were being opened for a downbound lockage. The leaf was driven upstream some distance against the high level of water; but, fortunately, the gate fell back, mitered partly with the opposite leaf, and, withstood the shock. Considerable damage was done, causing the lock to be out of commission for twenty-two days. Permanent repairs were made during the closed season.

In accident 3, the vessel struck the north leaf of the lower operating gate 3 ft from the miter post, the stem making a vertical dent 10 in. deep for 20 ft in the height of the gate. The north leaf passed the south one at the top of miter posts 3 ft to 4 ft, the water pouring in a large stream between the gates. These gates were out of commission the remainder of the season. Only the intermediate gates were used during this time, with the effect of shortening the lock by 93 ft. The repairs were made by the owners of the vessels during the closed season.

In accident 4, the steamer struck the south leaf of the upper operating gate, twisting the gate so badly that it could not be used the remainder of the season. The gate was temporarily replaced by a leaf of the upper guard gate. The lock was out of commission for seven days. The gate was completely wrecked; the work of repairs involved the dismemberment of practically all

parts and the rerolling of the plates at the mill. Repairs were made by the vessel owners. The cost of repairs was extensive.

In accident 5, the vessel struck the south leaf of the upper operating gate, doing considerable damage. The gate was forced open against a high head, breaking the miter so that the distance between the toe cushions was estimated by the lockmen to be about 2 ft. Fortunately, the gates mitered perfectly when they were closed. The gate was damaged considerably.

In accident 6, the vessel struck the north wall with her anchor about 200 ft east of the upper operating gates, and scraped along the wall until the bowsprit rode up on the footbridge. The wooden bow of the steamer was wrecked for about 10 ft. The lower part of the wooden vessel was sufficiently decayed to make it a fairly effective fender or cushion.

In accident 7, the vessel backed into the intermediate gate which had a head of 3 ft on it. The pressure of the vessel's rudder broke the miter, moving the gate on its pintle support. After breaking the miter, the wheel of the steamer struck the toe of the gates, breaking off the four propeller blades. The damage to the gate was not extensive.

In accident 8, in the Canadian Sault Canal, the steamer *Perry G. Walker*, in approaching the lock upbound, rammed against the lower lock gate under a head of about 20 ft. The impact broke the miter, all the gates were carried away except the intermediate and guard gates, making the canal and lock an open raceway. The Canadian passenger steamer, *Assimmboria*, with one hundred passengers aboard was tied up in the lock waiting for the steamer *Crescent City*, an ore laden steamer, to enter the lock behind her. The *Assimmboria* was swept out of the lock, followed by the *Crescent City*. All three boats were damaged more or less by glancing blows when passing each other. The movable dam was swung across the canal and the flow through the canal was sufficiently checked to place a spare set of gates, which had been built a short time before the accident. These gates were stepped in place on June 13 and the lock was again put in commission on June 21, 1909.

All the wickets of the dam were lowered into place successfully, except one, which rested against an obstruction at the sills. That wicket became badly twisted and could not be operated. This gap was closed by stop planks. This movable dam is similar to that of the South Canal of St. Marys Falls Canal, Michigan, which was put out of commission when the canal was deepened in 1943. The Canadian dam (item 8, Table 1) is still ready for use.

Each of the accidents cited in Table 1 was the result of a misunderstanding of signals between the vessel masters and the engineers.

Colonel Hall criticizes the proposed movable dam on the premise that it will be expensive. The cost of such a structure should not be a deciding factor in determining the type of an emergency dam. In the paper it is stated that, because of the small quantity of steel and other metal required in its construction as compared to other forms of dams, the cost would be relatively less. The required weight of steel, and other metals, in the proposed dam is indicated as being about 215 tons, consisting largely of standard structural sections. In the construction of the swing-bridge, movable dam in the South Canal of the St. Marys Falls Canal, more than 2,100 tons of metal were used.

Colonel Hall questions the need for a movable dam, stating that "for most waterways the extra insurance is not worth the cost." If provisions are made by installing two sets of operating gates at each end of the lock, the chances of wrecking both sets of gates are very slight. Such an arrangement has been depended upon in certain localities, but it has not been the sole dependence at the Sault canals. As stated, all the accidents enumerated in Table 1 were the result of a misunderstanding of signals of the vessel masters and the engineers at the throttle, over which the lock personnel had no control.

The recess that houses the folded dam is protected by cross-walls which will prevent the passage of boulders and other large objects from entering this recess. Large debris pits are provided upstream from these cross-walls. The anchor troughs are fully covered by angle flanges attached to the top anchors, thus preventing any debris from entering these troughs when the dam is in the lowered position. If sediment should be deposited on any or all parts of the dam, the structure can still be raised through a considerable load of sediment because, in the raising, the upper parts which constitute only a small part of the total weight of the dam are moved first.

Sedimentation will affect any form of movable dam, whether it has only a sill or any other underwater connection.

Mr. Southworth refers to the dynamic affect of a swirl of water against the dam caused by a wrecked gate leaf hanging by its anchorage in a diagonal obstructive position. The proposed dam will be placed upstream of all operating lock gates, and a swirl produced would be downstream from the dam.

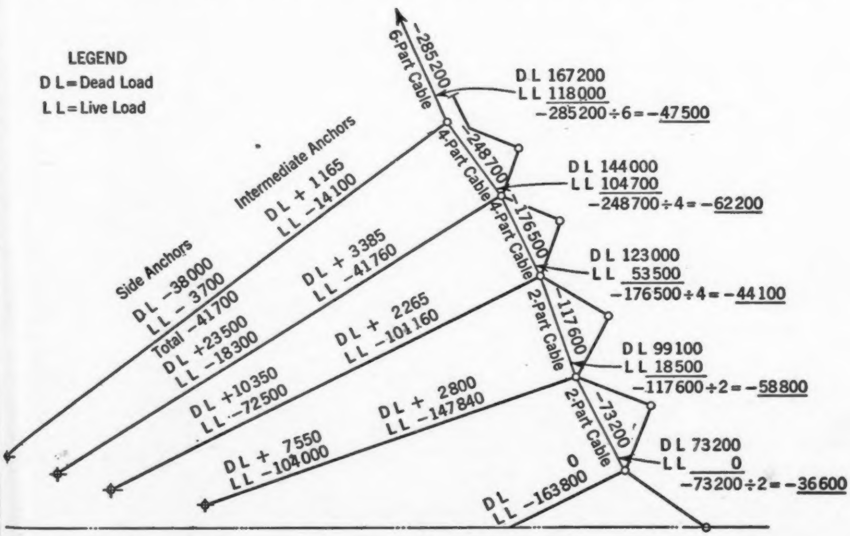

FIG. 8.—STRESSES (POUNDS PER SQUARE INCH) IN PRINCIPAL MEMBERS OF MOVABLE DAM

Regarding Mr. Sabin's criticism that the proposed dam has a large number of parts—there are nine plate girders and forty anchors, which form practically the entire structure, and the entire assembly is operated as one unit. In

comparison with the swing-bridge movable dam of the South Canal of the Sault canals it has, besides the bridge, thirty-two wickets and thirty-two wicket leaves. Each wicket and leaf is operated singly by cables through individual hoist mechanisms which are connected to a line shaft operated through motors located at the center of the bridge.

In the dam for the Davis and Sabin locks, there are two bridges to be lifted in place—the wicket bridge and the operating bridge. The dam is provided with thirty-two wickets, and into each wicket are to be inserted two 15-in. steel channels. Each of these units is lifted singly into place by the 85-ton derrick.

A model of the proposed dam has been made to determine its workability. It was made from plans in the early stages of its development and is not exactly in accordance with the latest plan, but the general characteristics and method of operation are the same. The operation of the model is very satisfactory.

Stress sheets have also been prepared for both the dead and live loads, which are based on a lock's having a depth of water on the sills of 30 ft, a width of 80 ft, and a head of water of 22 ft. Fig. 8 shows the stresses in the anchors and the cables. The stresses in the plate girders may be obtained readily from those indicated.